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THE PSYCHOLOGICAL REVIEW

GESTALT AND SIGN-GESTALT

BY EDWARD CHACE TOLMAN
University of California

In the present article I wish to point out some of the similarities and differences between sign-gestalts and true Gestalten. For sign-gestalts, as I have conceived them,[1] are, it would seem, only in some measure of the nature of the true Gestalten of Gestalt psychology proper.

Sensationalism, Perceptualism, Propositionalism. Perhaps the most significant way in which sign-gestalts can be set off, for purposes of contrast, from true Gestalten is to point out that sign-gestalts constitute the third term in a progression beginning with the sensations of Structuralism as the first term and passing through the Gestalten of Gestalt psychology as the second term. Or, to put it still another way: whereas Structuralism is a 'sensationalism' and Gestalt psychology a 'perceptualism,' Sign-Gestalt psychology is, I would assert, a 'propositionalism.' That is, whereas, according to Structuralism an organism responds to a complex of stimuli by a congeries of point-like 'sensations' and 'images' and according to Gestalt psychology by a configurated 'percept,' according to Sign-Gestalt psychology it responds rather by some total 'proposition' or 'propositions' (*i.e.*, by a sign-gestalt or sign-gestalts).

The 'percepts' of Gestalt psychology cannot we are told be analyzed into mere algebraic sums of sensations and images. For according to Gestalt psychology the organism always responds with an indissoluble 'group' or 'figure on ground' or 'step-up-phenomenon' or 'phi-movement' or whatever. Such a total percept includes, to be sure, those aspects of

[1] For a first statement concerning sign-gestalts see Tolman (10), especially Chapter IX and following.

quality, intensity, extensity, duration, which the sensationalistic Structuralist abstracts out and makes all-important. But these latter according to Gestalt psychology no longer stand alone, each separate in its own right. The organism never just senses—always it 'perceives.' But, turning now to Sign-Gestalt psychology, it will be asserted that an organism does not even just perceive, rather it always goes still further and 'propositionalizes.' And this proposition even though it contains the 'figure-on-grounds'—the percepts—of Gestalt psychology is never completely and without remainder analyzable into the latter. The 'figure-on-grounds,' etc., of Gestalt psychology are always caught up into some larger whole (*i.e.*, *sign-gestalts*),—containing signs, significates and means-end relations.

Before going ahead to expand this comparison, I must hasten however to point out that from a purely historical point of view it is not of course altogether fair. For just as there were undoubtedly features of the Gestalt psychology doctrine of 'perceptualism' already inherent in historical Structuralism so there are undoubtedly, and perhaps to an even greater extent, features of a sign-gestalt doctrine, or 'propositionalism,' inherent in present-day, *i.e.*, historical Gestalt psychology. My purpose in stating and enhancing the sequence is therefore not that of assigning or withholding credit, historically speaking, to or from actual schools but rather that of emphasizing and bringing to the fore a purely logical relation. Historically it may well be that there have never actually been any pure sensationalistic Structuralists nor any pure Gestaltists as I am respectively defining these two. There were forerunners of Gestalt-ism in historical Structuralism and there are very considerable forerunners of what I am calling Sign-Gestalt-ism in historical Gestalt psychology. It seems permissible, however, for purposes of logical clarity to imagine a pure sensationalistic Structuralism and a pure perceptualistic Gestalt-ism over against which to set a pure propositionalistic Sign-Gestalt-ism.[2]

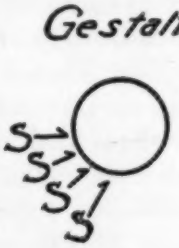
[2] This need for distinguishing between the contents of a doctrine as a mere 'geographical-historical' accident and the contents of a doctrine as a specific logical entity has been neatly pointed out by Lewin (2), p. 390. Unnecessary argumentation and confusion are sure to arise if such distinctions are not kept in mind.

Granting then that it is but such logically idealized schemes which I am presenting, let me offer the following three diagrams to indicate this interrelation between Structuralism, Gestalt-ism and Sign-Gestalt-ism.

Structuralism

Sensations (*s*) and images (*i*) combined in association by 'and' relations only.

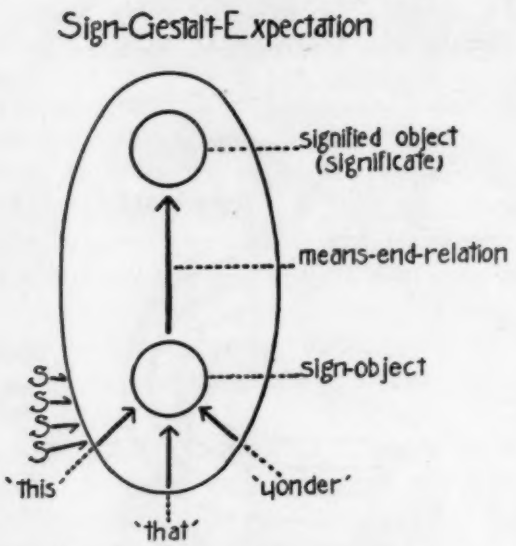
Gestalt psychology



The total circle represents sensory qualities together with 'shape,' 'movement,' 'figureness and groundness,' 'Sachlichkeit,' etc., all indissolubly bound up together into one configurational whole.

Let me illustrate by a concrete example. Consider yonder chair. According to the Simon-pure Structuralist (as we are conceiving such an hypothetical gentleman) the visual stimuli coming from such a chair will be thought to evoke in a human observer merely visual sensations plus some kinesthetic sensations from the eyes plus some tactual and kinesthetic images corresponding to the sensations which he would get from hand

Sign-Gestalt-ism



A sign-object located by some sort of a 'this' or a 'that' leading by a means-end-relation to a signified object.

and arms and legs if he were actually to touch the chair. According to a Simon-pure Gestalt-ist (again as we are conceiving such a one) these visual, kinesthetic and tactual stimuli will evoke a unique visual, tactual and kinesthetic configuration which will contain visual and tactual and kinesthetic qualities but will in addition present such properties as 'figure-on-groundness,' 'form,' 'solidity,' 'stationariness,' 'such and such internal accents,' etc. Finally, according to the Simon-pure Sign-Gestaltist (as we conceive the latter and naturally I am surest of him) these same visual stimuli will be thought to evoke not just this configuration as a mere 'perceived' given, but rather some specific larger whole in which this merely pictured (*i.e.*, perceived [3]) configuration will itself be

[3] It is obvious that throughout the discussion I am restricting the terms 'perceive,' 'percept,' 'perception,' etc., to only one of their several manners of being used. I am using them, that is, to cover only that which is given merely as a presented entity—a something with, to be sure, shape, form, etc., but quite divorced from any necessary further and 'practical' implications. Actually, of course, the textbooks tend to play fast and loose with the term perception. They usually begin by defining it in this

embedded as one term in a larger means-end 'proposition' such, as that: 'this chair, if sat on, will lead to rest (or its reverse)"; or "that chair, if placed against the wall, can be stood upon like a stepladder to reach this picture"; or "yonder chair, if placed near a table and sat upon, will make writing possible"; or "that chair over there, if placed near such and such other furniture, will form an aesthetically pleasing whole"; or "this chair, if kicked out of the way, will conduce to the catching of yonder escaped white rat"; and the like.

Discriminanda, Manipulanda, Means-End-Relations.—But in order to indicate more clearly and in more detail just what this progressive order between Structuralism, Gestalt psychology and Sign-Gestalt-ism really means it will be helpful to introduce some still further terms. And to this end I wish to use three new pairs, viz., 'discriminanda' and 'discriminanda-expectations,' 'manipulanda' and 'manipulanda-expectations, 'means-end-relations' and 'means-end-expectations.' [4]

Discriminanda and Discriminanda-expectations.—Discriminanda is my name for those properties of objects wherein, in purely sensory fashion, such objects differ one from another. This term is, in short, a substitute for the Sensationalist's term 'sense-qualities' (when those latter are conceived as actual existents in the objects). Discriminanda are investigated by arrays of sensory-discrimination experiments. By such experiments it is determined what the given environmental situation offers to the given organism in the way of *possible* sensory discriminations. Discriminanda are thus the *actual* discrimination-possibilities of the given environment which result from the nature of the environmental object and from the sense-organ make-up of the species of individual animal in question. Discriminanda are the actual sense-quality differentiations which, given the make-up of the environment and the sensory capacities of the organism, are

restricted way but are apt, then in subsequent use, to stretch it to cover what I am here calling practical (i.e., means-end) meanings as well. But it is for these practical 'means-end' meanings that I am suggesting the further term 'propositions.'

[4] For a previous discussion of discriminanda and manipulanda see Tolman (10) Chapter V, and for a previous discussion of means-end-relations and means-end-expectations see Tolman (10), Chapter XI and following.

actually there to be *enjoyed* in the given situation; if the organism will but do so.

Discriminanda-expectations, on the other hand, are the cognitive sets or determinants aroused in the individual organism on the given occasion. Such sets prepare the animal for there being such and such actual sense-qualities (discriminanda) then and there present (actually to be 'enjoyed'). But such expectation-sets may or may not be confirmed and supported by the actual facts of the environmental situation. They are not, for example, confirmed and supported in cases of illusion or hallucination. Discriminanda-expectations are, in short, my substitute for the Sensationalist's concept of 'sensations,' as mere subjectively aroused effects in the organism. (Just as discriminanda were my substitute for his concept of the sense-qualities actually present in the environment.)

It is to be noted further that the reasons why I prefer these terms (and concepts) discriminanda and discriminanda-expectations to the older ones of sense-qualities and resultant sensations are two. In the first place the concepts of sense-qualities and sensations appear to me undesirably and unnecessarily mentalistic and subjective. By discrimination and other experiments I can learn all there is to know both about the discriminanda which a given situation can present to a rat and also all about the momentary discriminanda-expectations which such discriminanda do actually arouse in him at the given moment but I can hardly be said to learn much of anything about the possible sense-qualities or his actual sensations (not, that is, if these terms are understood in their usual mentalistic way). And so also for a fellow human being,—I can learn all there is to know about another man's discriminanda and discriminanda-expectations by gross discrimination and other experiments (very often similar to those I would use with the rat but often differing, in that instead of involving gross behavior they involve merely verbal reports) and yet as a result of neither such method do I really learn anything about this other man's private mental 'feels.' [5]

[5] For an expansion of this argument concerning discriminanda vs. sensations see also Tolman (10) Chapters XVI and XXV. Also for what is essentially the same gument see C. I. Lewis (6) and (7), p. 76 f. and Rynin (9).

In a word, discriminanda and discriminanda-expectations are my names for and understanding of, sense-qualities and sensations *when these are defined objectively and in the same ways for both men and the lower animals.*

Secondly, I prefer the concept of discriminanda-expectations to that of sensations also because the former includes openly and avowedly the implication of 'objective reference'—an implication explicitly denied to 'sensations.' Sensations were conceived as nought but dead givens. They were not conceived as properly speaking in any way pointing outside themselves to the real sense-qualities. For me, on the other hand, the discriminanda-expectations have as part of their very warp and woof this pointing-outwards-ness. When a rat or a human being has the discriminanda-expectation of 'dark' he then and there 'expects' that this expectation is going to continue to be confirmed and supported by an actually present environmental situation—that, to wit, he can continue to 'enjoy' and have commerce with such a darkness. Such a darkness is not reacted to by him as to a mere fantasy of his own creation. It is responded to as to a in-some-way-objectively-caused darkness (even though when sophisticated he may realize that on occasion this objective cause is one residing wholly in his own physiology) which he can continue to 'enjoy.'

Turn now to our next pair of concepts.

Manipulanda and Manipulanda-expectations.—Under these two terms I would introduce two more—in this case almost completely new—notions. By manipulanda I would understand those properties of objects which actually support (*i.e.*, make possible) *motor manipulations.* And it is to be noted that, since the types of possible motor manipulation will vary with the structure of the species, one and the same objective environment often must offer quite different manipulanda to different species of organisms. Or, to put it another way, manipulanda bear the same relation to the motor-organ developments of species that discriminanda do to sense-organ developments. For just as its sense-organs will determine what discriminanda a species is capable of, the motor-organs

of a species will determine what manipulanda it will be capable of. One and the same environmental object will afford quite different manipulanda to an animal which possesses hands from what it can and will to an animal which possesses only a mouth, or only a bill, or only claws. Graspableness, pick-up-ableness, throw-ableness, heaviness (*i.e.*, 'heave-ableness') and the like—these are manipulanda. And it is obvious that they will be present in different measure and in different fashion for species which possess different equipments in the way of prehensile and locomotor organs. The environment will offer quite different actual ranges and orders of grasp-ableness, heaviness (heave-ableness), and the like, to an animal with only a bill from what it will to one possessing hands. And it will present quite different manipulanda in the way of spatial directions and distances to an organism like a rat with a body length of only six inches but with four feet to what it will to a human biped with two legs, but with each of these some three feet long. Or, again, the same environment will present quite different ranges and orders of solidity,—*i.e.*, stand-on-ableness, rest-in-ableness, and the like—to a species with the muscles and the general form of a gorilla from what it will to puny man or to a mere finned fish.

Again, just as one can distinguish between discriminanda as the sensory qualities (defined in terms of the sense-organ capacities of the given organism) actually offered by a given environmental set-up and the momentary discriminanda-expectations aroused in the organism (which latter discriminanda-expectations may or may not correspond to such actually present discriminanda); so one must distinguish between the manipulation-possibilities, *i.e.*, the manipulanda, actually presented by such an environmental set-up and the momentary manipulanda-expectations (which may be more or less veridical) arising in the organism as a result of that set-up. Manipulanda-expectations, like discriminanda-expectations, are sets or preparations aroused in the organism, which may or may not be confirmed and supported by the actual entities truly present then and there in the environment.

Manipulanda-expectations and Gestalt.—Another point may

now be made. It must be pointed out, namely, that manipulanda-expectations as just defined seem to be closely allied to the very features which Gestalt psychology has especially brought to our attention, *i.e.*, to 'figure and ground,' 'form,' 'solidity,' 'moving-ness' (or its reverse), 'size,' and the like. Or, in other words, whereas our concept of discriminanda-expectations seems to coincide rather neatly with the orthodox concept of sensation-qualities such as color, temperature, odor, pitch, visual size, visual form, etc., our manipulanda-expectations seem to correspond more closely to those special new sorts of characters of *total* configurations which it has been the virtue of Gestalt psychology to indicate and bring to the fore. In short, what I am here asserting is that 'figure-on-ground-ness' and 'shape,' 'size,' *Sachlichkeit* (*i.e.*, solidity, etc.), are really in the last analysis manipulatable properties. I am asserting that 'figureness' is probably in the last analysis nothing but pick-up-ableness; that 'groundness' is similarly perhaps nothing but walk-about-on-ableness or reach-about-on-ableness; that 'size' and 'shape' are nothing but further specifications of pick-up-ableness; that 'solidity' is probably rest-on-ableness or obstructing-ness or heave-ableness (heaviness); that 'movements' are undoubtedly nothing but such and such types of run-after-ableness; and so on.[6]

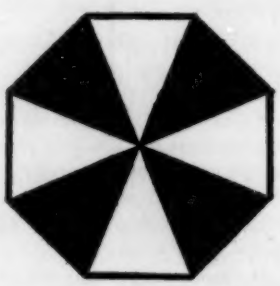
[6] Experimental evidence to support such a contention is, of course, wanting. I believe, however, that such evidence could be collected. What one would have to do would be, by experimentally controlling the larger means-end-relations (see below), experimentally to induce in the given subjects a determined upon manipulatory propensity. Then one would present these subjects with an ambiguous stimulus-situation to see if, as we are assuming, this induced manipulatory propensity would enter in and tip the scale so that the ambiguous stimulus-situation would now take on for the given subject the configurational property corresponding to this induced propensity. Let me illustrate by an example.

'Figure' and 'ground' as manipulanda-expectations are, I have just suggested, 'pick-up-ableness' and 'reach-about-on-ableness' respectively. To prove this experimentally I would attempt something as follows. First I would select an ambiguous figure and ground drawing such, for example, as the Rubin cross and choose two colors (perhaps black and white) such that in a large unselected mass of people there would be found an equal propensity when naive and untrained to see either the one or the other color as 'figure.' I would then select two large groups of observers and subject one of these groups to a preliminary training in which for a long succession of white and black stimulus-dots, presented tachistoscopically, they made for the white stimulus-dots picking up movements with respect to little objects presented in a container; while

Manipulanda (i.e., Configurating Properties) and 'Primary Qualities.'—Another point concerning manipulanda must now also be noted. Manipulanda, or as we have just further identified them the configurating properties of Gestalt psychology, are also, I believe, closely related to the 'primary qualities' of English empiricism. Thus, for example, Locke's list of the 'primary qualities' comprised, it will be remembered: solidity, extension, figure, motion, number, and situation [*i.e.*, spatial position?] (8). But are not these, I would ask, almost identical with the graspableness, the heave-ableness, the run-after-ableness, the sit-on-ableness, etc., which have just been defined as manipulanda (or configurating properties)?

The so-called primary qualities and the so-called secondary qualities are of course, in the first instance, both mere discriminanda—*i.e.*, mere stimulus-qualities to be differentiated so and so. They are nevertheless different from one another in that the primary qualities do, I believe, tend to exhibit a more consistent and persistent set of correlations with manipu-

for the black stimulus-dots they made merely pushing away movements with respect to these same little objects. And the other group I would subject to just the reverse training. That is, I would have these latter pick up for the black stimuli and push away for the white. I would then expose the Rubin cross for a brief instant tachistoscopically to both groups. And, if my experiment were successful, I should expect that



the group trained to pick up for the white stimulus-dots would see a white cross as figure on a black background and that the other group, exposed to the reverse training, would see a black cross as figure on a white background.

So much for a mere suggestion. Similar experiments could, I believe, be worked out for other types of configurational property—*i.e.*, for 'grouping,' 'shape,' 'size,' 'solidity,' 'movingness,' etc., which would show that these also, as expectations in the organism, are nothing but manipulanda-expectations,—*i.e.*, expectations, or sets, in the organism whereby he is prepared to grasp, to heave, to chase, to push against, and the like in such and such ways.

landa than do the secondary qualities. The primary qualities (*e.g.*, the visual and tactual and kinesthetic discriminanda of size and shape and resistance) do, that is, seem to have much more consistent and persistent correlations with such manipulanda as specific pick-up-ablenesses, sit-on-ablenesses, stand-on-ablenesses, heave-ablenesses, and the like, than do secondary qualities such as color or odor or temperature.

These differences are, however, ones of degree only. For sometimes a color or an odor or a temperature may be quite as persistently and consistently correlated with a given set of manipulanda as is a size or shape. Trees, as a given relatively definitive group of manipulanda, are, that is, almost always 'green.' And they are about as consistently green as they are consistently of any persistent *visual* and *kinesthetic* and *tactual* size or shape or resistance. The difference between the primary and the secondary qualities is thus, in the last analysis, due merely to the fact that it was easier for God, or the devil, to smear on and off colors, odors, tastes, temperatures without upsetting the underlying manipulanda than it was to smear on and off different visual and tactual and kinesthetic shapes and sizes and resistances. (10, p. 89f.) [7] But this is the only difference. And it is, as we have seen, one of degree only.

Means-end-relations and Means-end-expectations.—With these two terms I would now introduce still another pair of concepts. By means-end-relations I wish to designate those interrelational properties between environmental objects,[8] whereby the 'enjoyment of' (*i.e.*, the 'commerce with') the discriminanda and manipulanda constitutive of one such object will (or will not) actually lead on to an immediate possi-

[7] Some slight confusion must perhaps be guarded against in the reading of the argument here because the words, 'shape' and 'size' and 'resistance' seem to be used under ordinary circumstances to apply indiscriminately to what I am here specifically keeping separate, viz: to 'size' and 'shape' and 'resistance' as true manipulanda, *i.e.*, as defined in the last analysis in terms of 'stand-on-ablenesses,' 'heave-ablenesses,' and the like; and to size and shape and resistance as but the mere *visual* and *tactual* and *kinesthetic* discriminanda correlates of such manipulanda.

[8] I throughout use the term 'object' not only to apply to the relatively sharply outlined environmental entities for which the word is usually reserved but also to apply to those more amorphous, less distinct, environmental entities for which the term situation is more ordinarily adopted.

bility of 'enjoyment of' (commerce with) the discriminanda and manipulanda of such and such other environmental objects.

An organism immersed in his environment comes into contact at a given moment with a certain group of stimuli. As a result of these stimuli he 'expects' to be able to enjoy (to be able to have intercourse with) such and such a set of immediate discriminanda and manipulanda—or, as I have called it, such and such a 'sign-object.' But it is to be noted that in addition to, or rather simultaneously and circumambiently with, this expectation of a more or less immediately present sign-object, the organism has aroused in him a *further expectation* that this sign-object, if thus actually had intercourse with (*i.e.*, if actually enjoyed), will, or will not, lead on so and so to the presence of such and such further objects (sets of discriminanda and manipulanda)—or, as I have called these latter, such and such 'signified-objects,' or 'significates.' Such *expectations* of leading-on-ness (or not-leading-on-ness) are what I would define as means-end-expectations.

To sum up, it appears that it is the actual leadings-on or not-leadings-on,—including such subvarieties of this function as 'obstructing,' 'being an alternate to,' 'being a shorter way than,' or 'a longer way than,' or 'farther off than,' and the like, which constitute the means-end-relations; and it is the aroused preparatory sets in the organism for such possible leadings-on, or not-leadings-on, and their various subvarieties which constitute what I would call the means-end-expectations.

The environment as so envisaged is thus naught but a very field or tissue of means-end-relations. It is a *means-end-field* in which the various component objects and situations appear ineluctably in their rôles of possible, or impossible, good, or bad, better of worse, *means* to, or from, such and such other objects or situations.[9] Objects (and situations) provide easy or difficult routes (or perhaps, contrariwise, barriers) to or

[9] This concept of the environment as a means-end-field follows closely upon Lewin's concept (3, 4) of it as a 'psychobiological field.' And it is here in this conception of Lewin's that Gestalt psychology has probably most clearly and obviously already forestalled my Sign-Gestalt 'propositionalism.'

from other objects. And the defining 'easynesses' and difficulties' of these routes are of course dependent in the last analysis upon the given organism's make-up. They rest finally upon the functioning of some internal physiological principle of 'least effort.' Objects which present 'short' (*i.e.*, easy)routes do so because the given organism's physiological internal constitution so makes them. And objects which present 'long' (*i.e.*, difficult) routes do so because that is the way again that this same internal physiological mechanism of least effort affects them. The means-end-field and its constituent facts of distance and direction are superimposed upon some more fundamental underlying logic,—some logic of physiological 'least effort';[10] though as to the details of this latter we are, it must be admitted, now almost completely ignorant. Finally, however, it must be emphasized that even though this underlying mechanism determinative of means-end-characters be thus as yet unknown to us we are nonetheless at present able to go on discovering and asserting various facts and principles at the psychological or means-end-level itself.

Means-relations and Means-expectations versus Position-relations and Position-expectations.—It must be noted next that in the further analysis of this concept of the means-end-field it will be helpful to make a sub-distinction. We will, that is, find it helpful to distinguish two sub-varieties of means-end-relations. The one set will be called simply 'means-relations' and the other 'position-relations.' This distinction is a purely functional one. It arises out of the fact that, in so far as there is a means-end-field, in responding to any sign-object or signified-object in that field not only must the organism be sensitive to the character, or relations, of that object as providing a short or long (direct or indirect) 'means' to or from such and such other objects; but also he must be sensitive to the complementary 'position' of that object as a something which in its turn is to be got to or from. Any environmental object provides not only routes to or from other

[10] For bringing to the fore the need of this concept of physiological least effort we must thank both Gengerelli (1) and Wheeler (11).

objects but also presents concomitantly a complementary character of having its own 'position,' *i.e.*, the character of an object which in its turn may be easily, or difficultly, or so and so got to or from. Objects have 'means-relations' by virtue of the manner in which they afford routes to or from other objects, and they have 'position-relations' by virtue of the manner in which such and such other objects would provide routes to or from them.

Or, putting it another way, any object in a means-end-field is to be depicted as presenting to the organism both a set of 'means' lines radiating *from* it and a set of 'position' lines radiating *towards* it. A given object will be selected and responded to by the organism partly because this object appears to present short 'means' lines to some further more ultimately desired object and partly because it appears also to have short 'position' lines leading to the immediate presence of it itself. Such a distinction between the two kinds of lines is obviously, however, purely functional. It depends upon the side from which the given object is considered. As an object to-be-got-to means-end-relations are called 'position-relations'; as an object to be used to get to some other more distant object, means-end-relations are called 'means-relations.' This, however, is in the last analysis solely a functional difference.

In so far as the organism is responding to relations of how to get to an object, the organism may be said to be experiencing 'position-expectations' relative to that object. In so far, on the other hand, as the organism is responding to the relations involved in using the given object in order to get to or from further objects the organism may be said to be experiencing 'means-expectations' relative to the former object.

The Spatio-Mechanical Conception of the Means-End-Field.—There is one other point which must be stressed. This concept of the means-end-field as I have thus suggested it, is obviously built up very largely upon spatio-mechanical lines. I have been assuming, that is, that the means-end interrelations of objects to one another and to the organism may be

translated for purposes of the present psychological discussion primarily into simple spatial and mechanical symbols.[11]

And assuming such a spatial mechanical symbolization I would now suggest some six subvarieties of component variables. These I would name as: 'direction,' 'distance,' 'direct open path-ness,' 'obstructing-ness,' or, as I shall hereafter call it, 'barred-path-ness,' 'round-about-path-ness,' and 'tool-path-ness.' One object lies in 'means-end' terms relative to another first of all and most generally in such and such a 'direction' and at such and such a 'distance.' But such directions and distances will, it appears, always be more specifically qualifiable as 'direct' or 'barred' or 'round-about' or of the nature of a 'tool.' The means-relations and the position-relations are in the first instance directions and distances but secondly such directions and distances will further specify themselves in terms of their 'directness' and 'openness' or their 'barredness' or their 'round-about-ness' or their 'tool characters.'

Direction will, in general, be that property inherent in a means-end-field whereby a certain set of means-objects arrange themselves into some natural order or sequence relative to their 'goodnesses' or badnesses' as presenting routes to or from specified further objects. Those means-objects which afford the better,—*i.e.*, the shorter, routes will be said to point in the more 'direct' directions; whereas those which afford the poorer, *i.e.*, the longer, routes will be said to point the less direct directions.

And *distance* will thus be a correlative concept to that of direction. In any type of means-end set-up, distances will be the property which is expressed by the relative *preferabilities* of the various alternative directions. The more preferable directions will present the 'shorter' distances; the less preferable, the 'longer' distances.

A *direct open path* in any means-end set-up will be defined as one without obstacles—*i.e.*, as one in which the type of activity necessary to reach the further object is direct and constant in kind (*i.e.*, constant in direction).

[11] It is to be pointed out that again it was Lewin (See 3 and 4 as well as 5) who first pointed the way to such a spatio-mechanical envisagement.

A *barred path* in contrast will be defined as one in which some change in 'direction,' *i.e.*, in the type of 'activity,' is necessary to get on. A barred path will be one which requires a 'round-about path' to circumvent it.

A *tool-path* will be defined as one in which intercourse with one object brings with it willy nilly intercourse with some other object so that this latter intercourse brings the organism into the presence of the desired further object (just as intercourse with the one end of a lever causes the other end to achieve such and such another desired intercourse).

Examples of Means-End-Relations Represented in Spatio-Mechanical Terms.—In order to make both the possibility and the desirability of such a spatio-mechanical symbolization of many various types of means-end-relations clearer, it will be helpful to give some concrete illustrations.

The first and most obvious type of case will be that in which the actual set-up is in truth spatial and mechanical. In such an instance the various objects in the given means-end environment will be removed from the organism and from one another in actual spatial and mechanical ways. But it is to be emphasized that even in such a case it will probably turn out that the means-end distances and directions do not actually or exactly correspond with the under-lying spatial distances and directions. For example, it has been found that rats when well motivated by hunger will prefer (*i.e.*, choose) the shorter of two paths to food and in the same degree apparently to which actually they can differentiate between those two paths in terms of the spatial manipulanda which they experience in running over them. Under such conditions they exhibit Weber's Law.[12] That is, they will differentiate and prefer any path if it be approximately one-tenth shorter than the other alternative path. In such a case means-end distances and spatial manipulanda distances probably correspond. Suppose, however, to assume a purely hypothetical case, never yet as far as I know actually tried out, that the rats be run with considerably less hunger, in such a distance discrimination set-up. I would now bet that under such con-

[12] See Yoshioka's experiment (12) for an extremely careful and elaborate investigation of this point.

ditions their choices of the shorter path will not be so refined and will not correspond to their previously discovered discriminanda and manipulanda capacities. I am betting that now they will accept as equivalent (*i.e., as far as their means-end purposes go*) paths which are not identical for them in spatial discriminanda or manipulanda—*i.e.*, which differ from one another in actual spatial lengths by considerably more than one-tenth. Assuming such an outcome, then we should have to say that, whereas as far as the rats spatial discriminanda or manipulanda go an increase in spatial distance of one-tenth would continue to constitute a true change in spatial discriminanda and manipulanda, that under conditions of less motivation, such a spatial change need not involve a corresponding means-end change. Under less hunger a path which as a set of spatial discriminanda and manipulanda can be differentiated as somewhat shorter or longer from another path may, from a *means-end* point of view, be accepted as exactly equal in length to that other path.

Or, consider a hypothetical case with a human being. Suppose the matter be one of selecting one among a number of almost equally good means-objects—say pencils. And suppose that half of these pencils be at actual spatial distances anywhere from three feet to three yards from the individual whereas the other half of them are at spatial distances of one hundred yards or more. In such an instance it seems to me pretty certain that, as far as means-end distances would be concerned, the individual would exhibit no distinction between the three-foot-distant pencils and the three-yard-distant pencils. All the pencils within these two extremes would probably for most means-end purposes be grouped together as 'near' as contrasted with the group of one-hundred-yard distant pencils which would be classed as 'distant.' And yet as far as actual spatial discriminanda and manipulanda were concerned this individual would of course still be able to distinguish between the three-foot-distant and the three-yard-distant.

So much for examples to suggest that even when the actual discriminanda and manipulanda stuffs of the given

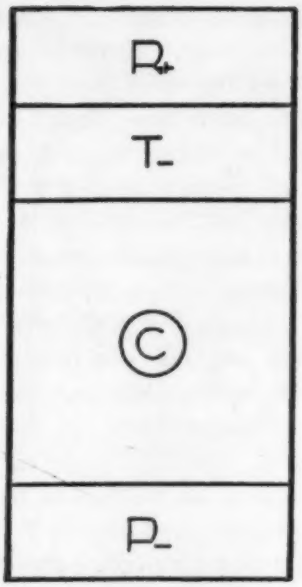
set-up are primarily spatial what we are calling the means-end distances need not be the same as these discriminanda and manipulanda distances.

Consider now, however, a type of instance in which the actual stuff of the situation is not really spatial at all. Social situations are particularly good examples. Lewin has presented many illustrations of such social situations with young children and he has shown that these situations also can be fruitfully translated into spatio-mechanical diagrams.[13] For example, he draws the following picture to represent the case of a child who is offered a reward by an adult for the performance of an unpleasant task—together, at the same time, with a threat of punishment if he does not perform the task. *R* is the reward; *T*, the undesired task; *C*, the child; and *P*, the threatened punishment. Lewin thus conceives the situation as one in which the child is placed in a 'field' in which the task and the threatened punishment are in opposite 'directions.' To 'go towards' the task is to 'go away from' the punishment and vice versa. Again, the reward is drawn as 'lying beyond' the task as it can be reached only by 'going through' the task. The heavy lines bounding the whole rectangle are 'barriers' which 'enclose' the situation, in the sense that, due to the power of the adult, the child can't 'escape out of' doing either the one or the other. Finally the degree of freedom 'open' to the child before it makes up its mind to do either the one or the other is represented by the 'size' of the middle 'area' in which the child is thus represented as 'confined.' If this area is drawn as small, it means that the choice is very imminent and the child can have little free-play before making up its mind, that is, before going over into the 'area' of either the task, *T*, or of the punishment, *P*. Whereas if this middle area is drawn larger, it means that the child will have a much 'bigger' opportunity to play 'about' before doing either the task or receiving the punishment.

In such an illustration it is quite obvious that the true discriminanda and manipulanda stuffs of the situation are not by any means primarily spatial, and yet it is equally obvious

[13] Adapted from Lewin (4), p. 54.

that such non-spatial, but social, materials can be quite readily translated into the simple spatial and mechanical concepts of areas and barriers, etc., placed so and so relative to one another.[14]



Finally, however, even though in the end it should turn out that these spatio-mechanical diagrams and concepts are not adequate for certain and all types of means-end feature, and even though some other more comprehensive type of representation must finally be found, nonetheless it still seems desirable for us now to go on pushing these spatio-mechanical concepts as far as we can. For whatever we succeed in stating in these terms now will undoubtedly be more readily translatable in the future into whatever other terms may then have to be substituted for them.

[14] An examination of the figurative building up of language suggests that such a spatial transformation or analogy has always played an instinctive part throughout the history of thought. Thus, for example, in the paragraph above it was only by putting in quotation marks that I felt I could bring it sufficiently home to the reader that I was here using the words in question in their explicitly *spatial* sense because one and all of these words are so commonly used in means-end discussions without stopping to realize their actual spatial significance.

Recapitulation: Discriminanda, Manipulanda, Means-End-Relations.—To sum up, according to Sign-Gestalt psychology, environmental objects offer not only discriminanda and manipulanda but also means-end-relations. And these means end-relations may for convenience be subdivided further into 'position-relations' radiating towards a given object and 'means-relations' radiating from that object.

The stimuli from such environmental objects therefore tend to evoke in the organism not only discriminanda-expectations but also manipulanda-expectations and means-end-expectations (this last subdivided as we have seen into position-expectations and means-expectations). Not only does the organism 'sense' such and such objects as presenting such and such discrimination-possibilities, but further he also 'perceives' in these objects concurrently such and such correlated manipulation-possibilities. And, thirdly, he also concomitantly 'propositionalizes' in them such and such associated means-end possibilities,—*i.e.*, that they present such and such 'positions' as entities to be got to or from and likewise that they present such and such 'means' as paths for the getting to such and such other entities.

Finally, as a Last Word.—It is to be emphasized that all three kinds of expectation are interactive, conjoint and mutually inter-affecting. That means-end-expectations determine manipulanda-expectations and that the latter determine discriminanda-expectations is often almost as true as the reverse. Indeed it has been the function and thesis of Gestalt psychology to indicate the second step—*i.e.*, the partial dependence of the component discriminanda-expectations upon the characters of the including and circumambient manipulanda-expectations. So it shall now in analogous fashion be the function and thesis of Sign-Gestalt psychology to emphasize the former step—viz., the, at least partial, dependence of the manipulanda-expectations in their turn upon the including and circumambient means-end-expectations. That sign-gestalts determine Gestalten is just as true and important as that the latter determine 'sensations.'

REFERENCES

1. GENGERELLI, J. A., Preliminary experiments on the causal factors in animal learning, *J. Comp. Psychol.*, 1928, **8**, 455–458; 377–384.
2. LEWIN, K., Gesetz und Experiment in der Psychologie, *Symposion Phil. Zsch. f. Forsch. u. Aussprache*, 1927, **5**, 375–421 (Berlin, Schlacthensee, Weltkreis Verlag).
3. LEWIN, K., Die psychologische Situation bei Lohn und Strafe, Leipzig, S. Hirzel, 1931, pp. 67.
4. LEWIN, K., Environmental forces in child behavior and development. Chapter 4 in A Handbook of Child Psychology, ed. by C. Murchison, Worcester, Mass., Clark University Press; London, Oxford University Press, 1932, pp. 94–127.
5. LEWIN, K., Untersuchungen zur Handlungs- und Affect-psychologie. I through XI, *Psychol. Forsch.*, 1926–1932, **7**–16.
6. LEWIS, C. I., The pragmatic element in knowledge, *Univ. Calif. Publ. Philos.*, 1926, **6**, 205–227.
7. LEWIS, C. I., Mind and the world order, New York, Charles Scribner's Sons, 1929.
8. LOCKE, J., Essay concerning human understanding, Book II, Chapter 8.
9. RYNIN, D., The nature of communication, *J. Philos.*, 1932, **29**, 505–515.
10. TOLMAN, E. C., Purposive behavior in animals and men, New York, The Century Co., 1932.
11. WHEELER, R. H., The science of psychology, New York, Thomas Y. Crowell Company, 1929.
12. YOSHIOKA, J. G., Weber's law in the discrimination of maze distance by the white rat, *Univ. Calif. Publ. Psychol.*, 1929, **4**, 155–184.

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ERRORS IN THE CRITIQUES OF GESTALT PSYCHOLOGY IV: INCONSISTENCIES IN WOODWORTH, SPEARMAN AND McDOUGALL

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I

In addition to Thorndike's system, which was reviewed in the preceding number of this series,[1] there are three other important ones, of a functional character, that directly face the problem of unity: those of Woodworth, Spearman, and McDougall. Woodworth's system approaches this problem from the standpoint of biological functionalism; Spearman faces it by emphasizing the creative aspect of mental life, and McDougall, the purposeful aspect. None of the authors under consideration finds in configurational principles a solution of his respective problems. All three resort, in the end, to mechanistic theories either entirely, in the form of associationism, or indirectly, in the guise of a vitalism. Woodworth's inconsistencies lie in an eclecticism which forces upon him a great many unnecessary self-contradictions. Spearman's inconsistencies lie in the assumption that an associative system solves the problem of creativeness. McDougall's inconsistencies, like those of James, lie in an artificial mechanistic-vitalistic dualism. The mind functions purposefully in the course of making a blind machine, the body, function teleologically.

II

It will not be our purpose to answer directly Woodworth's criticisms of *Gestalt* psychology [2] but rather to answer them

[1] PSYCHOL. REV., 1933, 40, 303–323.

[2] Contemporary schools of psychology. New York: Ronald Press, 1931, Chapter 4.

indirectly by analyzing sources of confusion. These sources are selected from his text book, *Psychology*.[3]

There are many evidences in Woodworth that demonstrate his sympathy toward configurational psychology. There is a realization of wholes and the part they play in behavior. For example, " . . . psychology takes the individual as a whole" [4] and, "An individual without a social environment is as unreal an abstraction as an individual without organs." [5] But this verbal approach to the organismic position is more superficial, however, than it sounds, for we are told that we must, in the end, "attempt to gather up the threads in a synthetic study of personality." [6]

Woodworth does not accept the consequences of 'the individual as a whole.' Without a vigorous reliance upon relativistic modes of thought a consistent use of his concept would be impossible, and Woodworth is not thinking relativistically. There are several differences, for example, between human and animal behavior. "Man makes more use of his past experience . . . man adapts himself better to novel situations." [7] These are typical illustrations of absolutism, an absolutism that goes with machinism and atomism. Comparing man in an absolute sense with the animal, he does make more use of his past experience, but such a statement is misleading. There is no evidence that, in proportion to his degree of neural differentiation, man makes more use of his past experience than the animal, or that he adapts himself, better, to novel situations. Woodworth's assertions are important because they demonstrate just the point of view which makes it difficult to grasp configurational principles. The problem of unity is a corollary of relativity. The one implies the other, and unless the necessity is felt of thinking relativistically, the problem of unity will be faced mechanistically instead of organismically, and a misunderstanding of *Gestalt* psychology will inevitably result.

[3] New York, Henry Holt, 1929 (Revised edition). Hereafter referred to as W.
[4] W, 3.
[5] W, 4.
[6] W, 19.
[7] W, 24.

In searching for a physiological explanation underlying intelligence Woodworth starts with two sets of incompatible assumptions. These give him trouble throughout the book. He attempts to be mechanistic and configurational at the same time. "If each little part of the brain does its job promptly, the whole brain works together as a team, while if the parts lag, the team work of the whole is imperfect. . . . If each player gets instantly into action, the team as a whole responds to the pattern of the situation; but if the players lag, some doubtless lagging more than others, there is no coördinated response to the situation as a whole." [8] Notice that throughout this quotation Woodworth is thinking of parts first and wholes second. He is attempting to explain how the integration of the whole depends upon independent contributions of the separate parts. The team, as a whole, responds to the pattern of a situation only when the total act is built up piecemeal. The question immediately arises, How could each 'player know what to do,' whether to lag or not to lag, how much to contribute and how much not to contribute? It makes no difference to the individual player, whose activity is mechanical and therefore directionless, whether it lags or not; by definition its performance has nothing to do with the direction of the total act. The rôle which each little part of the brain must play depends upon its relation to the whole. This is not theory but fact. Woodworth has the facts reversed. Some 'players' lag more than others because a dynamic condition in the brain as a whole, equivalent to the purpose of the whole, compels them to lag, otherwise there is no reason for them to lag or not to lag, no reason for them to function at all.

In his chapters on Memory and Learning, Woodworth repeatedly divides the organism against itself. The organism puts '. . . two and two together to build up a solution' [9] but at the same time it is possible 'that mental power increases by a process of natural growth.' [10] Thus development is of two kinds, the one a consequence of maturation and the other a consequence of associational learning. Woodworth does not

[8] W, 67.
[9] W, 73.
[10] W, 72.

face the obvious possibility that 'associational learning' is nothing more than a consequence of maturation, and that what seems to be trial and error learning is simply behavior whose maturation is incomplete, and hence the responses are gross and 'inadequate.' Woodworth himself cites cases in which trial and error behavior was eliminated by a control of maturation.[11] What does this mean if not that 'trial and error' is a product, not an explanation, of learning, where maturation is not under control? Like Thorndike, Woodworth accepts a more complicated and less logical interpretation in place of a simpler and more logical one. The concept of 'trial and error' persists as a tradition, and nothing more.

Maturation and insight are mutually dependent phenomena, a fact which Woodworth realizes, but the rôles that both play in behavior are ambiguous. ". . . we should not attribute foresight and purpose to the animal, if his behavior can possibly be understood as driven by hunger, or thirst, or some other organic need."[12] Why shouldn't we? Woodworth's answer is: "We lose the scientific value of the simpler types of behavior if we immediately interpret them as complex."[13] Woodworth has not sensed the changed interpretation of the law of parsimony which goes with the configurational position. To assume that the simplest types of behavior are atoms of response which must be put together is certainly not the simplest assumption when it raises the unsolvable problem of explaining what puts them together, or in other words, what creates something from nothing, such as a relationship between the atoms that did not exist before. If the atoms of behavior *were* related at the outset, then maturation at any given level of development *is* the simplest assumption and the only necessary one. To grant an undifferentiated and simple form of insight to the animal is a much simpler view, therefore, than the mechanistic one. Indeed, it is the typical error of 'machinism' to mistake simplicity for complexity. A mechanical is far more complicated a situation than an organismic one.

[11] W, 208.
[12] W, 132.
[13] W, 132.

From what has just been said we can expect confusion in Woodworth's treatment of the learning process. It appears in many places. "Trial and error learning is learning by doing, and not by reasoning or observing." [14] This statement is denied later. "The characteristics of trial and error behavior are that it is aimed at some goal. . . ." [15] Woodworth in trying to be consistent, but under false assumptions is forced to contradict himself not only once but twice in the same sentence. We have just noted one of the self-contradictions. The other follows in the same sentence, thus: ". . . [trial and error behavior] is relatively blind and unobservant. . . ." [16] Nothing could be more unintelligible than a performance which is at the same time blind and aimed at a goal. Such confusion of necessity keeps the discussion of learning at the tautological level, thus: ". . . learning consists in going through with the activity to be learned." [17] "The cat learns to get out by getting out, not by seeing how to get out." [18] Either learning is just learning, or, when the cat learns to get out of a puzzle box all that he is doing is to get out; in reality he is not learning.

The confusion continues into the chapter on Heredity and Environment, where it is said that, "Maturation prepares the raw materials for learning. It provides weak activities that are strengthened by use, and variable activities that are regularized by use. It provides smaller activities that are combined by learning into larger action patterns, and it provides broad, diffuse movements that are broken up by learning into more minute and precise skilled movements." [19] In the first part of the second sentence Woodworth mistakes the facts. He states them more correctly in the second half of the sentence although it is not learning that differentiates the diffuse movements. *Learning is the differentiation.* This error illustrates a typical one always made under mechanistic

[14] W, 138.
[15] W, 167.
[16] W, 167.
[17] W, 89.
[18] W, 138.
[19] W, 208.

assumptions. The same event is given two names, or in other words, made into two events, the one supposed to be the cause of the other. And again the mechanistic point of view violates the law of parsimony.

These are not the only difficulties with the quotation just cited. How can maturation, a phenomenon based upon the dynamics of unified fields, prepare the raw materials for learning when learning, in Woodworth's own view, presupposes the opposite situation than that of a unified field? Under Woodworth's assumptions maturation can have no efficacy whatever.

The same mixture of incompatibles appears again. "Learning thus comes in to a slight extent to perfect the unlearned movement, while leaving its organization unchanged." [20] But by Woodworth's own definition of learning, which is associational, there must be a change in organization. In other words a change in organization cannot even to a slight extent improve the unlearned movement and leave its organization unchanged. Once more: "Skilled movements are combinations and specializations of unlearned movements." [21] How can a movement be a combination and a specialization at the same time? The two states, if achieved, would be by incompatible procedures, the one beginning with the parts and chaos, the other beginning with the whole, and unity. The assumption of both at the same time rests upon two opposite definitions of unity, namely, derived and axiomatic unity, when in reality it is the same unity all the time.

Woodworth seems to abandon his mechanistic position at brief intervals. "What you are doing in toto determines what you do piecemeal." [22] But if this is true, the mechanistic position is wrong, and it is the mechanistic position which prevails throughout the text. Woodworth's concept of the nervous system makes impossible the activity which he has just conceded. Consequently, he is forced again into the self-contradiction: "A complex act or series of movements is likely

[20] W, 208–209.
[21] W, 220.
[22] W, 238.

to be purposive as a whole, and directed towards an anticipated result, while the single movements that make up the total act are mechanical. . . ." [23] Unless each single movement occurred in the light of the remote end toward which the act as a whole was progressing how could it ever have occurred? In admitting that the act as a whole may be purposive, he is granting the directional character of each component movement. In order to be relevant to the total performance the rôle of a part must be played in such a way that it helps bring the total act to its anticipated end.

The same discrepancy appears in Woodworth's treatment of perception. "Perception is a combining activity, as well as selective." [24] "The whole that is perceived gives advantage to parts coherent with that whole." [25] "If all the elements fall readily into a single group, that pattern has the advantage. . . ." [26] In other words, if for some unknown reason the previously existing elements form into a pattern, the pattern can then function in its own right. The whole that is perceived has no efficacy after all; it depends upon being built up from its parts, when, at the same time, we are told that in its own right it can give advantage to its parts. How can a whole, where parts are primary, also be a whole where parts are secondary? This is what Woodworth is trying to tell us.

Can Woodworth's criticisms of *Gestalt* psychology be taken at their face value when the position which he offers as a substitute is as confused as this? Is eclecticism a legitimate goal of science when the price paid for it is self-contradiction? Is it worthwhile to be in the middle of the road at the expense of inconsistencies and mis-statements of fact?

III

While Spearman's psychology, from a phenomenal point of view, presents an entirely different picture, its inconsistencies are the same. A mechanistic background will no more

[23] W, 245.
[24] W, 391.
[25] W, 392.
[26] W, 390.

permit solving the problem of creativeness than it will permit solving the problem of learning. The problem of learning is a problem of creativeness.

Spearman gives us three basic qualitative and five basic quantitative principles. The first qualitative principle is stated as follows: "*Any lived experience tends to evoke immediately a knowing of its characters and experiencer.*" [27] In another statement of the same law, verbally at least, an entirely different meaning is indicated. "*A person tends to know his own sensations, feelings, and strivings.*" [28] In these two statements of the principle there is an implicit self-contradiction. In the first statement, which is precisely James' position, a particular experience brings to light knowledge of the experiencer. A part introduces the whole. In the second statement of the principle, a person as a whole introduces the parts, the experiences. The first statement is mechanistic; the second is organismic. But Spearman does not accept the organismic implication of his own statement, for he chooses as the more exact formula the first of the two quotations.

The second principle is called the eduction of relations and is formulated as follows: "*The mentally presenting of any two or more characters tends to evoke immediately a knowing of relation between them.*" [29] In restating this principle Spearman inadvertently puts the matter configurationally, thus, "*When two or more items (percepts or ideas) are given, a person may perceive them to be in various ways related.*" [30] Again, this is unsatisfactory to Spearman because he returns, in a footnote, to his original statement. In his explanation of the law it is not a person that relates the items; two discrete mental processes, having at first no relation between them, generate a relation, or, at least, are the conditions under which a relation is in an unknown way manufactured out of whole cloth. There is no whole, no field phenomenon within which all items

[27] The nature of intelligence and the principles of cognition, London, Macmillan, 1923, 48. Hereafter referred to as S1.

[28] Creative Mind, New York, Appleton, 1931, 16, (S2).

[29] S1, 63.

[30] S2, 18.

are already related, whereby the structurization of *A* and *B* within the whole can be effected with the relation, *C*, coming into existence at the same time. Spearman is as much as saying that a given set of circumstances produces what corresponds to a high potential and also what corresponds to a low potential, but fails to produce the differential between them, namely, their relation. Somehow the high and the low, the *A* and the *B*, of their own accord must produce the differential, the relation *C*. Had Spearman accepted the implication of his own statement, "a person may perceive them," the story would have been an entirely different one, for the person represents an already integrated whole. His mind represents an already integrated ground from which items emerge and the relations between them in one unified response to a multiple cause. The items do not come first and the relations second either actually or logically; *A*, *B* and *C* emerge together.

Spearman's third principle is called the eduction of correlates: "*The presenting of any character together with any relation tends to evoke immediately a knowing of the correlative character.*" [31] A later statement of the principle involves the same curious approach to configurational psychology, but is again taken back: "*When any item and a relation to it are present to mind, then the mind can generate in itself another item so related.*" [32] Spearman would have met less difficulty had he followed through his second statement instead of retracting from it. As it is, one of two dilemmas arises at once. The item and its relation must repeat the same impossible performance that is assumed in the second principle, or else the eduction of a correlate is nothing more than a matter of memory and is not creative at all. We have item *A* and the relation *C*, given. The problem is to explain the emergence of the item *B*. A situation induces what corresponds to a high potential and at the same time a differential, *but without the other item, the low potential.* There is therefore nothing, no item *B*, for item *A* to be related to in terms of the relation *C*.

[31] S1, 91.
[32] S2, 24.

Obviously item *B* must exist somehow. Therefore, if we are to follow Spearman's own assumptions, it must either be in the nervous system or in the subconscious. In either case we have nothing creative. Item *A* and the relation *C* suggest an already experienced item *B*. If an organismic situation had been assumed in the beginning, the principle of correlates would have turned out to be nothing more or less than the configurational principle of transposition. The wonder of it is that Spearman should ever have attempted to build up a complex situation from elements in the light of the admission: "The concentrated essence of the whole is the first thing to come up in our minds, because it is the one thing that was growing while every other thing was being thought." [33]

The first of Spearman's quantitative experiments deals with mental growth. "*Every mind tends to keep its total simultaneous cognitive output constant in quantity, however varying in quality.*" [34] If one resorts to the concept of energy one immediately faces the facts of energy which are opposed to Spearman's principle. It is not that an energy field tends to keep its total output constant. On the contrary, it tends to preserve the balance of whatever energy it possesses, and the amount of energy it possesses is always derived from its dynamic relation to its surroundings. There is no reason for assuming that cognitive output does not follow this law. If it does, the output is not constant; [35] it varies with conditions.

The second quantitative principle reads as follows: "*The occurrence of any cognitive event produces a tendency for it to occur afterwards.*" [36] Here Spearman makes the typical mechanistic (atomistic) error of dividing a continuum into sequences of events and assuming that one step in the sequence explains its successor. In reality, the second step in the sequence depends upon the first step only by definition, as was pointed out in Number III of this series. The cause of misunderstanding concerning the necessity of traces lies in the

[33] S1, 82, quoted from Henderson, *Psychol. Monog.*, 1903, 23.

[34] S1, 131.

[35] Cf. a discussion of this point in Principles of mental development, Wheeler and Perkins, New York, Crowell, 164 ff.

[36] S1, 132.

unnecessary assumption that an experience must be represented by a particular neural action-pattern existing when stimuli do not impinge from outside. Spearman assumes traces, obviously. According to the law of parsimony they should not be assumed because they are unnecessary. All we need, as far as the nervous system is concerned, is a general level of differentiation whose particular action-patterns exist only as stimulus-patterns cause them to exist. For example, the reason why a congenitally blind man cannot visualize is because he cannot see; it is not because he has not seen. The second event is not caused by the first or produced by it, but by a part of the same stimulus-pattern that produced the first event. Similarly, habits do not persist because they are repeated; they persist because something external to them is making them persist.

A third quantitative principle states that, "*The occurrence of any cognitive event produces a tendency opposed to its occurring afterwards.*" [37] Again Spearman has the cart before the horse. Is it that one occurrence produces fatigue and thus renders repetition less likely, or is it that a single set of circumstances is causing a progressive change? Spearman's quantitative principles are supposed to be energy principles but energy does not behave in the way in which he implies. The change that causes the fatigue is the same as that which induces the response. It is not the response itself that causes the fatigue. Physiologically, the appearance of toxic products, so-called, is as much an aspect of the response to a stimulus as that aspect which does not include the toxic products.

Fourth, "*The intensity of cognition can be controlled by conation.*" [38] Spearman recognizes that his system offers little if any reason why this should be true, although he considers it the most momentous of all his principles. "To avowed ignorance about this law, rather than to any deficient appreciation of its importance, then, must be attributed the inadequate part that it plays in the following pages." [39] And

[37] S1, 134.
[38] S1, 135.
[39] S2, 33.

yet a principle in which he professes little faith would explain very simply why it is truly one of the most momentous of all psychological laws. The problem of the will is a complete mystery under either mechanistic or vitalistic assumptions, but as a phenomenal property of a unified field of energy, the organism as a whole, it is easily understood.

Finally, one enjoys a picture, we are told, by appreciating the constitutive relations first, and second, *a unity which emerges out of these relations.*[40] Like James, Thorndike, and Woodworth, Spearman thus puts the parts first and attempts to derive his unity from them, in spite of the fact that he makes use of material (principally from art and music) showing precisely the opposite state of affairs. Spearman's difficulty arises out of the assumptions which dictate his statements of the facts. His principles are merely restatements of association psychology, and his effort to explain creativeness commits the same synthetic fallacies as were found in the other authors we have been considering. The configurational substitute for the eduction of relations and correlates has already been mentioned in our answer to Thorndike. It may be repeated here briefly. Just as the same total situation in one act induces a high and a low potential and also the differential between them, so *A* and *B* are experienced together, with the relation between them, in one act, a configurational response to a multiple stimulus-pattern. This is the well known phenomenon of insight which Spearman grants but attempts to explain mechanistically. Once more it is seen that whenever one attempts to construe unity as a derived phenomenon he does so at the expense of absurdities.

IV

Spearman and McDougall diverge from a background of British conation psychology, the first with a marked Scholastic influence and a strong leaning toward the psychology of the Würzburg school, the second with a biological functionalism in general resembling that of James and Angell. As a typical example of McDougall's position we take *The Outline of*

[40] S2, 57.

Psychology.[41] In many respects McDougall is more in harmony with *Gestalt* psychology than any other psychologist under discussion but, like James, he is confronted with an impossible problem arising from his original assumptions. He supposes that the biological organism functions mechanically and the psychological organism teleologically. It is inevitable, therefore, that the discrepancy be accepted and that the system as a whole become a strongly vitalistic one wherein mind intervenes and directs a machine. Nevertheless, McDougall's psychology and organismic psychology have in common an emphasis upon insight and purpose. There are, however, fundamental differences. *Gestalt* psychology avoids associationism altogether, as well as the dualistic discrepancy between mind and body. It gives to the dynamics of the body the same organismic interpretation that it gives to mental functions. In short, it is monistic while McDougall is dualistic.

We commence with McDougall's seven marks of behavior characteristic of organisms from the lower animals to man. First, there is '*a certain spontaneity of movement*.'[42] McDougall must grant spontaneity to behavior when, by implication, physical activities do not possess this property. If the mind is to intercept and direct the functions of the body when it is not a machine like the body, it must by assumption possess freedom. *Gestalt* psychology grants freedom to mind and also concedes the same phenomenon to physical events under the same principles of dynamics.[43]

The second mark of behavior is '*the persistence of activity independently of the continuance of the impression which may have initiated it*.'[44] This principle plays the same rôle of providing freedom and spontaneity to the psychological organism. It harmonizes with *Gestalt* psychology to the extent that the latter grants a similar independence, but for an entirely different reason, a dynamic one. This is the principle, already mentioned, that the first and second steps in a se-

[41] New York, Scribners, 1923. Hereafter referred to as M.
[42] M, 44.
[43] Cf., Wheeler, Laws of human nature, 114, 195 ff.
[44] M, 44.

quence are not related to each other in terms of cause and effect but have definitional dependence. Both steps depend, as a unified temporal configuration, upon external factors.

The third mark of behavior is a '*variation of direction of persistent movements.*' [45] Through volition and foresight the organism can modify its mechanical habits to suit its needs. Contrary to McDougall's assumption,[46] configurational psychology has by no means slighted the phenomenon of foresight when, by definition, insight into a goal can be nothing more than foresight.

Fourth, there is a '*coming to an end of the animal's movements as soon as they have brought about a particular kind of change in its situation.*' [47] McDougall has no means of explaining the efficacy of ends or purposes in his vitalistic scheme. He must either leave the question unanswered or define purpose as an endowed power directing a mechanical performance toward a given end. A conative process directing itself toward an end by means of an innate property is no better than a mystical agent. In the dynamics of unified fields, configurational psychology finds a solution of this dilemma under the well known and observable conditions for least action.

A fifth mark of behavior is '*preparation for the new situation toward the production of which the action contributes.*' [48] The same statement would be made in organismic psychology. It has been made as the law of configuration. The organism responds as a whole, relationally, to a total situation. There is no one-to-one correspondence between a given part of the stimulus and a given part of the response. The organism makes a contribution to the situation, in terms of its unified, insightful reaction. Thus the action, as McDougall affirms, helps to make the situation for the reacting organism.

Sixth, is '*some degree of improvement in the effectiveness of behavior, when it is repeated by the animal under similar circum-*

[45] M, 45.
[46] Insight and foresight in various animals, *J. Comp. Psychol.*, 1931, 11, 239.
[47] M, 45.
[48] M, 45.

stances.'[49] Both psychologies admit degrees of improvement but in explaining improvement they diverge immediately. In the absence of a substitute, McDougall must fall back upon association psychology.

The seventh mark of behavior is purposive action, which '*is a total reaction of the organism.*'[50] Verbally this sounds organismic but actually it is not. The components of the total reaction are, as Woodworth makes them, mechanical, while the total reaction is purposeful. One must again fall back upon a mystical agent that organizes purposeless parts into a purposeful whole. Organismic principles elude this dilemma by showing how the so-called mechanical activities of the nervous system obey the same laws of dynamics as do the purposeful phenomena of mental life. Physical movement is directed toward remote ends. Purposeful behavior is directed toward remote ends. The difference is phenomenological, not functional. A falling body, for example, is under stress toward the remote end, the earth's center. A conscious organism is under a corresponding potential toward its goal and the phenomenal property of this tension, or neural stress, is an awareness of the goal. There is no mysticism here. We simply have a case of nature's most common phenomenon, a resolution of a stress in accordance with the law of least action. The movement of a part within the whole, the 'player' of Woodworth's simile, is not mechanical; each nerve impulse is directional; it has a remote end as does the response as a whole, for the same principles that apply to the organism and its environment, a part within a whole, apply to the nerve impulse and the organism, a part within a whole. There is no 'blind' or 'directionless' assemblage of part-activities mysteriously summing to a directional, purposeful totality.

McDougall not only fails to make intelligible how a non-mechanical mind functions with a mechanical body; he fails to protect the hormic or teleological aspect of his system from the inroads of 'machinism.' The hormic processes themselves, as simple phenomena, become organized into wider and more

[49] M, 46.
[50] M, 56.

complex systems.[51] They are not, after all, less mechanical in the end than are physical processes, although they are said to distinguish living from dead material. If both dead and living systems come into existence by a combining process, just as machines are made, how are they different? Throughout the book, parts are given prior existence and must be put together. Thus a "relating synthetic activity is the essence of perception."[52] Yet, like James, McDougall struggles against the mechanistic implications of synthesis. "I see no reason why . . . complex coördinated movements should be regarded as the expressions of so many distinct instincts."[53] And further, "any one instinctive impulse may make use of a variety of motor mechanisms."[54] Shortly, the task of abandoning synthesis is given up and it is formally accepted. There is no other way out; the problem has been determined by a mechanistic-vitalistic dualism and "*habits are only instruments, which subserve our purposes, but do not determine them.*"[55] Instinct or propensity is the driving power in habit. The instincts or insightful activities that direct the habits are in the end so many separate processes which, in turn, must be put together in the same fashion as were the habits themselves.

Further evidence of McDougall's fight against machinism, with nothing but mechanistic logic to help him, can be found in his temporary refusal to accept such a principle as the law of effect. "The behavior of the animal mother who fights to the death in defense of her young is only the most striking illustration of the ineptitude of the hedonist theory of action."[56] In speaking of the glutton's so-called eating for the sake of pleasure, he remarks, "The answer of the hormic theory is that such instances only arise as the result of a long process of sophistication; that primarily what the glutton desired was to eat food; that gradually, by exercise of the power of abstraction, he learned to distinguish the pleasure that accompanies

[51] M, 73.
[52] M, 76.
[53] M, 116.
[54] M, 118.
[55] M, 181.
[56] M, 193.

eating from the eating itself, and to emphasize this aspect of the total experience of eating." [57] In this the configurationist heartily agrees. But the law of effect is a law of association and McDougall subsequently accepts the laws of association. His denial of hedonism gets him nowhere in the end.

Dissatisfied with James' account of perception because of its obvious concession to machinism, McDougall charges James with misinterpreting Lotze in the latter's statement of the 'psychic stimulus' theory. According to McDougall, Lotze's local signs were not sensations, or for that matter, any mode or quality of experience.[58] McDougall accepts the local sign as a purely nervous process in order, apparently, to save the perceptual consciousness from being a mosaic. But what does it matter if in turn the nervous system must be a mosaic? It is curious that both Lotze and McDougall sense the fact that eye movements follow 'the shortest route' in bringing stimuli to bear upon the fovea from the periphery. Had the organismic implications of this fact been apparent to them, the position-property or local sign of any given spot on the retina would have been construed as a derived instead of an innate phenomenon. From then on it might have been clear that the psychic stimulus problem was a configurational one, namely, the problem of how a unified response can be made to a multiple stimulus without a one-to-one correspondence between stimulus and response.[59] Both Lotze and McDougall rely upon the conventional theory of habit for an explanation of 'local signs.' A specific quality develops in a given spot because the function of that spot is always the same, in a stereotyped activity. Under *Gestalt* assumptions the local quality emerges when its function is the same in no two movements that are alike. It emerges in the course of continuous transposition, playing a part first in one whole (pattern of eye movements) and then in another, the opposite of the habit theory. In the first case habit gives the retinal spot an innate property. In the second, transposition brings

[57] M, 271.
[58] M, 241.
[59] M, 381.

with it a derived property common to an enormous variety of wholes.

McDougall is at one time on the point of solving his whole problem. ". . . mental growth by association and accretion, which is an inevitable corollary of the mosaic or atomistic psychology, really inverts the true order of events. It describes mental development as beginning with the cognition of particulars and advancing to cognition of the more general; whereas, in reality, we begin by cognition of the highly general and progress gradually to the cognition of particulars." [60] But a growth, in terms of which differentiation and expansion are the only possible methods, is subdivided into three processes none of which is true differentiation. These three processes are discrimination, 'apperception' and 'association.' [61] Discrimination, the most hopeful of the three, succumbs to association on the next page. "The cognitive disposition by means of which he [a child] at first perceived both kinds of fruit [apple and tomato] has become differentiated into two; of these two, the one corresponding to apples continues to be connected with the conative disposition that impels to eating; the other, that which corresponds to tomato, has become connected with the conative disposition of disgust. Thus the child has learned to discriminate through the bad consequences of his failure to discriminate; and in this his progress is typical of all advance of discrimination; we learn by making mistakes, as we say." [62] The wholes with which we start are not organismic wholes that undergo differentiation. They are, like Titchener's genetic units, something to be dissected, in order to ascertain how they were put together from parts in the first place. They were put together by a discovery of so many separate differences between objects, bonded together according to the law of association by contrast, and the law of effect, which latter McDougall had practically repudiated earlier in the text. Thus his process of discrimination resembles organismic differentiation only verbally.

[60] M, 381.
[61] M, 382.
[62] M, 383.

As we proceed, the associationistic view of discrimination becomes even more clearcut. "The process of differentiation . . . requires to be rectified by another process, if the mind is to acquire a high level of effective organization; and this other process is apperception." [63] Apperception, according to McDougall, is the process of discerning similarities between objects and classes of objects.[64] This process, in turn, capitulates to association. ". . . a child is familar with the eggs of animals and with the seeds of plants; but has never thought of their similarities, until he is lead to do so by hearing the word 'egg' applied to the seed of a plant, or the word 'germ' applied to both eggs and seeds. From this time on, the word 'germ' will be used by him to mean the properties common to both eggs and seeds." [65] Apperception comes about, therefore, through the conventional process of association by similarity. McDougall seems to be unaware that he has thus let in by a back door that which he threw out the front door. There was nothing else to do, other than to invent a configurational theory. This would have preserved the original unity of the whole with which, in his own terms, he had commenced. As it is, like James, he destroyed it. The destruction achieved, he was logically compelled to admit it, and this he does, for albeit that discrimination and apperception produce the 'logical structure of the mind,' [66] as if association had been avoided so far, the mind would have no knowledge of the historic order of events unless "historical knowledge is built up by association. Association binds together the dispositions of the logical structure by a system of cross-connections which mirror the historical relations as perceived." [67] A vigorous battle is lost and we return to that mechanistic realm to which habit had originally been conceded. "It may be that the difference between true mental association and habit formation is only a matter of difference of level in the nervous system." [68]

[63] M, 386.
[64] M, 386.
[65] M, 387.
[66] M, 392.
[67] M, 392.
[68] M, 394.

Although "the structure of the mind is full of . . . associative links between cognitive dispositions; and these links reflect the temporal sequences of its past experiences . . . association begins to work at a relatively late stage of development only." [69] The 'later stage' is not in reality a later genetic stage. The 'postponement' of association is only an illusion brought about by the assumption of larger 'dispositional' rather than smaller 'ideational' atoms. Under McDougall's own definition, association must operate between the first and second experiences of *any* genetic sequence, in order that their temporal or 'historic' relation can be explained. It is hardly a 'later stage,' therefore, when it must begin functioning at once. In configurational psychology, wholes are space-time phenomena. No association is required to explain temporal relations. Two processes differentiate already related temporally, as well as spatially. There is no process of forming a bond between them. Hence association does not enter the picture to make differentiation the opposite from that which it really is.

Aquinas and James erroneously thought that they had avoided the mechanistic import of association, the one by making the parts universal and the other by making them objects. McDougall commits the same error of shifting the scene instead of the logic. "The mistake of the association psychology was to assign to association also the work of making 'ideas'." [70] "Association makes nothing new; it merely connects . . . dispositions previously formed, previously differentiated by discrimination and logically ordered by apperception." [71] Of what avail is this when association is surreptitiously employed to explain both discrimination and apperception? Had association really not been there in the beginning, it would not have crept in later. The principles that would have saved the first story of McDougall's edifice from association would also have saved the second.

[69] M, 395.
[70] M, 395.
[71] M, 395.

V

We have seen in the systems of James, Titchener, Thorndike, Woodworth, Spearman and McDougall, psychologies that attempt to handle unsuccessfully the problem of organization. The psychologies of Titchener, Thorndike, and Woodworth, insisting upon the forming of connections, under laws of association and attention, can never be made over into configurational psychologies. Under the assumptions with which they start, regarding the problem of science and the nature of wholes, they cannot approach, even by the smallest steps, toward configurational psychology without plunging into self-contradictions. Attempts at eclecticism are of no avail because the original assumptions of these psychologies preclude an eclecticism. James' fallacies will be repeated until fundamental assumptions are changed. An effort, such as we find in Spearman, to cover over the self-contradiction of machinism with the principle of noegenesis is of no avail so long as noegenesis begins, mechanistically, with the parts. In these four systems there is only one logical framework, that of atomism, which is machinism *par excellence*.

The three major problems, emphasized, respectively, by Woodworth, Spearman and McDougall, are biological functionalism, creativeness, and purpose. All three are by assumption fundamental in organismic psychology, and all three are explained in terms of principles dealing with an axiomatic unity within which, not outside of which, there is a plurality. Association and attention are not in the picture, either in the beginning or at the end. Biological functionalism, creativeness and purpose are all subsumed under one set of laws, the laws of dynamics interpreted as laws of unified wholes.

Some among these writers, James and McDougall in particular, start with the apparent conviction that association and synthesis are unsound, mechanistic principles. But eventually the principles are dragged in. Wrong assumptions will not solve problems no matter how intensively the problems are felt.

Historically, association and synthesis were conceived as means of deriving order from chaos. They are tools of

mechanistic thought. They can never be anything else. They are efforts to explain a derived unity, and wherever employed, they are incompatible with axiomatic unity and the principle of differentiation. One cannot have differentiation without an organismic field, otherwise the differentiated structures have no relation, or no reason for differentiating.

The mechanistic and organismic systems are both all-or-none systems. They begin on opposite sides of the part-whole dichotomy. The assumptions necessary for the one are precluded by the other. There is no carrying of water on both shoulders. An eclectic or middle position is impossible. One or the other is totally wrong. It is a choice between them.

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A THEORY OF THE ACTION OF THE AFTER-EFFECTS OF A CONNECTION UPON IT

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One of the objections to the hypothesis that a satisfying after-effect of a mental connection works back upon it to strengthen it is that nobody has shown how this action does or could occur. It is the purpose of this article to show how a mechanism which is as possible physiologically as any of the mechanisms proposed to account for facilitation, inhibition, fatigue, strengthening by repetition or other forms of modification, could enable such an after-effect to cause such a strengthening. I shall also report certain facts and hypotheses concerning the work which this mechanism has to do and the way in which it seems to do it. These are of value regardless of the correctness of my identification of the mechanism itself.

For convenience we may use symbols as follows:

N = the neurones of an animal.

B = the rest of the animal's body.

C = any activity, state, or condition of N.

S = any situation or state of affairs external to N considered as a cause of some C.

R = any response or state of affairs external to N, considered as a result of some C.

By a satisfying state of affairs or satisfier is meant one which the animal does nothing to avoid, often doing things which maintain or renew it. By an annoying state of affairs is meant one which the animal does nothing to preserve, often doing things which put an end to it.

A satisfier exerts an influence that strengthens any modifiable C upon which this influence impinges. Not knowing what Cs are made of, or how a strong C differs from a weaker

form of the same *C*, one must speak in figures and analogies. The influence may thus be thought of as like an addition of current or potential, or a decrease of resistance, or an intimacy of connection, or a continuance for a longer time.

The *C*s upon which it impinges most will be among those which have recently been active or will shortly be active. That is, the action of a satisfier is conditioned by its place in the succession of *C*s.

The *C*s upon which it impinges will be preferentially those situated in the part or feature or pattern or system or organization of *N* in which the satisfier occurs. When an animal that runs about seeking food attains it, the strengthening will be more likely to influence the *C*s concerned with its locomotion, its hunger, and its ideas about food and eating, than those concerned with contemporaneous casual scratchings of an itching ear, or stray thoughts about Shakespeare's sonnets or Brahms' symphonies.

More narrowly the influence will impinge preferentially upon the *C* (or *C*s) to which the satisfier 'belongs' as a part of a more or less unitary group of *C*s, or larger *C*. In the animal just mentioned, the satisfier will strengthen the *C* between reaching the doorway to the food-box and going in, more than the *C* between reaching that doorway and pausing to inspect it. The excess strengthening will be far more than the slight difference in time can account for. If, in an exercise in completing a word, say oc·re, by supplying a missing letter, a person tries first *a*, then *e*, then *i*, then *o*, and then *h*, being rewarded by 'Right' for the last, and then at once proceeds to look at the next word, the satisfier will strengthen the *C* with *h* enormously more than the next preceding or following *C*, far more than the removal by one step and a second or so could account for.

Its influence will not, however, pick out the 'right' or 'essential' or 'useful' *C* by any mystical or logical potency. It is, on the contrary, as natural in its action as a falling stone, a ray of light, a line of force, a discharge of buckshot, a stream of water, or a hormone in the blood. It will strengthen not only the *C* which is the most preferred according to the

principles stated above, but also to some extent *C*s which are wrong, irrelevant, or useless, provided they are close enough to the satisfier in the succession of *C*s.

One naturally asks first whether the action of a satisfier may be by stimulating the general circulation and thus causing the *C*s which happen to be in a state of excitement at or near the time of occurrence of the satisfier to be preferentially strengthened by some metabolic process. The facts seem to deny this possibility. The strengthening influence of a satisfier is probably in the form of a reaction of the neurones themselves. It is too rapid to be via an increase or decrease in the general circulation, or by the liberation of a hormone. When a series $S \rightarrow R \rightarrow$ Reward or Punishment, $S \rightarrow R \rightarrow$ Reward or Punishment, $S \rightarrow R \rightarrow$ Reward or Punishment is run at the rate of 3 seconds per unit, the action of each satisfier is localized at and around its point of application in the series with almost perfect clearness. And this is approximately true with rates of 1½ seconds or even 1 second per unit. Moreover, remoteness in steps seems (though the data are not yet adequate) very much more important than remoteness in time in restricting its application.

This unknown reaction of neurones which is aroused by the satisfier and which strengthens connections upon which it impinges I have called the 'Yes' reaction, or O. K. reaction, or confirming reaction. Though its intimate histological basis and physiological nature are no better known than those of facilitation, inhibition, fatigue, strengthening by repetition, or any other forces causing temporary or permanent modifications in *N*, certain facts about it are known in addition to those already stated concerning its causes and results.

The confirming reaction is independent of sensory pleasures. A pain may set it in action, as Tolman, Hall, and Bretnall have recently demonstrated in a striking experiment.[1] The confirming reaction, though far from logical or inerrant, is highly selective. It may pick out and act upon the words one is saying, leaving uninfluenced one's posture and gross bodily movements and all that one is seeing.

[1] E. C. Tolman, C. S. Hall, & E. P. Bretnall, *J. Exper. Psychol.*, 1932, 15, 601–614.

The confirming reaction seems often to issue from some overhead control in *N*, the neural basis of some want or 'drive' or purpose or then active self of the animal. This overhead control may be rather narrow and specific, as when a swallow of liquid satisfies thirst, and the satisfaction confirms the *C* which caused the swallowing, and makes the animal continue or repeat that *C*. This may happen while the main flow of his purposes concerns the work he is doing or the game he is playing or the book he is reading. It may be very broad and general, as when the purpose is to do well and win a game or to pass the time pleasantly, and is satisfied by any one of many movements in response to some play of one's adversary or by attentiveness to any one of many sights and sounds. It may be stimulated to send forth its confirming reaction by a rich sensory satisfier, such as freedom, food, and companionship for an animal escaping from a cage, or by a purely symbolic satisfier, such as the announcement of 'Right' in an experiment in learning. If what the overhead control wants is the announcement of 'Right,' that is what will most surely lead it to make the confirming reaction.

As suggested by the preceding paragraph, several wants or purposes or controls may be operative at the same time or in close alternation.

Arrangements may be made whereby certain events acquire power to cause the confirming reaction in the absence of anything that would ordinarily be called an overhead control. The reward or satisfier may then exert the confirming reaction directly upon the *C*.

If a $S \rightarrow R$ connection has a satisfying after-effect which causes some control in the *N* to send forth a confirming reaction, and if the *S* continues, the confirming reaction tends to cause a continuance or continued repetition of the *R* then and there, and often with more vigor and shorter latency. If the situation has vanished, the strengthening of the *C* can only manifest itself when *S* recurs, which may be in a few seconds or only after months. There will then be an increased probability of repetition over what there would have been if no confirming reaction had affected the *C* in question. In either

case the strengthening causes the repetition, not the repetition the strengthening.

The potency of a confirming reaction may bear little relation to the intensity of the satisfier. A 'want' or 'purpose' or 'self' may be as well satisfied, and so issue as full and adequate a confirming reaction, by a moderate reward as by one much larger. There seems to be an upper point beyond which increases in a reward add only excitement. Toward the low end there is a range where the reward fails more and more frequently to arouse an adequate confirming reaction. There seems to be a point below which a confirming reaction is not evoked. A state of affairs below this degree of satisfyingness is satisfying to the extent of being tolerated, and nothing is done to abolish or evade it, or to replace the *C* which caused it by some other *C*; but also nothing is done to strengthen the *C* and continue it longer than it would otherwise have been continued, or to repeat it in the future more frequently than it would otherwise have been repeated.

At the other end of this neutral zone begin states of affairs which are annoying to the animal and stimulate him to do whatever his repertory provides as responses to the annoyance in question. His repertory does not provide a general destructive or weakening reaction which is comparable and opposite to the confirming reaction, and which subtracts from the *C* upon which it acts. Any apparent subtraction is due to the increased strength of competing tendencies. The annoyer does not then and there destroy or weaken the connection of which it is the after-effect, but only causes the animal to make a different response to the *S* in question.

I do not think that this tendency to do something different in response to an *S* the first response to which has resulted in an annoying state of affairs, is a unitary tendency applicable to any *C*, and replacing it indifferently by any other *C* than it. The confirming reaction set in action by a satisfier, has, if my observations are correct and adequate, no comparable altering reaction set in action by an annoyer. The reactions in the latter case seem specialized and closely dependent on what the annoyer is and what state the *N* is in.

Whether or not this is so, an annoying after-effect of a certain $S \rightarrow R$ has very different possibilities *according as the S remains or vanishes.* If it vanishes, the annoyer can do nothing, because it cannot change the response to an S which is not there. So, in multiple-choice learning in which each S vanishes as soon as it is responded to, punishments have zero influence upon learning and punished connections may do more harm to learning by occurring than they do good by being punished. If the S remains and the response to it is changed, the animal may benefit from the fact of changing, and from the occurrence and the after-effects of the $S \rightarrow R_2$ which has replaced $S \rightarrow R_1$.

What sort of force acting through what sort of process or mechanism can be and do what the confirming reaction is and does? The answer which seems to me to fit all or nearly all the facts is that the force and mechanism of the confirming reaction are the force and mechanism of reinforcement, applied to a connection.

All explanations of reinforcement agree that one part of N can exert a force to intensify activities elsewhere in N, and that processes or mechanisms exist whereby this force can be directed or attracted to one activity rather than promiscuously; and that is all that is required to explain the fundamental physiology of the confirming reaction. It is distinguished from other sorts of reinforcement by the fact that satisfaction sets the force in action and that the force acts on the connection which was just active in intimate functional association with the production of the satisfier, or on its near neighbors.[2]

[2] The differences between the present theory of the action of after-effects upon connections and that suggested by the writer twenty years ago should perhaps be mentioned. The older theory, though possibly true so far as it went, paid insufficient attention to the positive reinforcement of a connection as contrasted with the mere leaving it undisturbed. It also was inadequate to explain the spread or scatter phenomenon whereby unrewarded or punished connections are strengthened if they are in close enough proximity to a rewarded connection.

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PSYCHOLOGY AND FREUD: AN HISTORICO-CRITICAL APPRAISAL [1]

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In the thirty odd years of its existence psychoanalysis, to an even greater degree than has been the case with other so-called 'new' psychologies, has been the victim of lampooning ridicule as well as the recipient of ecstatic praise. In fact, the evidence for and against psychoanalytic claims is so confused and charged with emotional bias as to render critical appraisal a dubious and difficult undertaking. Our main purpose in this article is not to sift this evidence in any detailed fashion, but to place the controversy in an historical setting that will furnish a more adequate perspective in terms of which the clash between academic psychology and Freudianism can be subjected to dispassionate scrutiny.

At the beginning of the psychoanalytic movement its adherents stressed therapeutic efficacy as confirmation of Freudian theory. For reasons which need not detain us here, this type of evidence has been pushed into the background of late. The principal contribution is regarded as a scientific revelation in the realm of pure psychology as opposed to medicine. Freud (6, p. 673) himself is apparently of this opinion, for he writes: "The future will probably attribute far greater importance to psychoanalysis as the science of the unconscious than as a therapeutic procedure." Heinz Hartmann (7), one of Freud's disciples, has written a book for the purpose of reconciling the psychoanalytic ideology with the canons of scientific method demanded by academic psychology. Psychoanalysis is thus demanding a hearing as a system of psychology and not merely as a psychotherapeutic technique.

[1] This article owes much to contacts made by the writer at Vienna during his work there as a fellow of the Social Science Research Council during 1930–1931. He desires to record his gratitude to the Council for having made these contacts possible.

Disregarding the latter for the time being, it may prove illuminating to consider the former demand in the light of traditional psychological teaching.

The Freudian approach to mental phenomena is unique in the history of psychology in at least one respect: it is the product of what might be called the autobiographical approach. It is not a 'brass instrument' psychology, nor a mental test psychology. So far as its methodology is concerned, it fails to fit into the traditional pattern of either rational systems as developed by Leibnitz or of empirical systems as initiated by Locke. Certainly it cannot be characterized as a systematic introspective psychology either in terms of the strict Leipzig tradition or of the less rigid Würzburg precedent. Taking a broad, tolerant, sympathetic attitude toward Freud's work, we think he merits credit for being a pioneer in this autobiographical attack on mental life.[2]

Although it is very likely an outgrowth of the medical case history, the Freudian approach, to the best of our knowledge, represents the first attempt to found a system of psychology on the basis of the common man's story of his life. And irrespective of our disapproval and skepticism of the resulting system itself, the possibilities of the method may be far richer than is ordinarily suspected. We are not speaking of the psychoanalytic method in the sense of a search for the etiology of psychoneuroses. This phase of the method with its dream symbolism, polymorphous perverse sexuality, repressions, and the whole of the psychoanalytic edifice is secondary to the phase to which we refer. It will be well not to let our attitude toward such specifically Freudian terms blind us to the possible value of the psychological method initiated by Freud and on the basis of which he developed his system. Of course by this time the original method in orthodox psycho-

[2] Both James (11) and Starbuck (17) should be mentioned among the pioneers in the use of biographical material even though the material was delimited to religious experiences. An interesting non-Freudian German extension of the utilization of the biographical approach to psychological problems is exemplified in the recently published work of Charlotte Bühler (2). Murphy and Murphy (14, p. 570) should be consulted for a discussion of the problem of biographical methods in connection with personality measurement.

analytic circles has been considerably modified by the theoretical accretions of the system. But originally, and this is the sense in which we are using the term autobiographical method, the patient told the story of his life in the greatest possible detail. It was not a cursory summary of the sort recorded by the hospital interne in less than an hour with sole emphasis upon a few easily catalogued features. Hall's questionnaire method represents the adaptation to psychology of this approach in terms of particular biographic features. The uniqueness of Freud's original approach is to be found in its theoretically total absence of specificity. Starting at any point at all the subject was encouraged to tell all he could regarding himself. There was no time limit and, in theory at least, no particular phase of life emphasized more than any other. The goal was to understand the factors responsible for the sweep of mental phenomena as experienced by academically unsophisticated men and women. To attain this goal all phases of experience were potentially of equal promise: the subject might tell of his early struggles with square root, his fondness for velvet ties, his uncle's arthritis, his present enthusiasm for Wagnerian music, Scandinavian philology, and the four Marx brothers, not to mention his competence as a chess player and his mother's disapproval of bicycling.

In a way this procedure is akin to the devices employed by the writer of so-called psychological novels or fictionized biographies. The mind of man is presented in familiar, everyday descriptive categories. There is a directness and an intimacy and relevance far different from our professional attempts to do something scientific with the problem of personality.[3] A Gamaliel Bradford or an Emil Ludwig can present a more effective personality analysis than the technically trained academic psychologist. As we indicated in a previous paper,[4] this difference may be due to the futility and irrelevance, for the purpose of understanding human nature, of much of our technical psychology. After all, when confronted with the

[3] Consult the monograph by Allport and Vernon (1) for an excellent bibliography on these attempts.

[4] This journal, 1932, 39, 552–569.

problem of understanding the personality of a Charles Darwin or of an obscure deputy sheriff, to talk of Lissauer's tract, the helicotrema, laryngeal movements, obstruction boxes, the law of parsimony, and the semi-interquartile range is to introduce pathetically inadequate conceptual tools with which to cope with the problem. What constitutes the particular merit of Freud's autobiographical method is its directness of attack. The data secured are not fictitious as in the case of the novelist, but a record of events as recalled by individuals ostensibly influenced by the events in question. Whether the events have been distorted in the telling is assuredly important, but of no profound adverse consequence so far as the psychological promise of the method itself is concerned. To understand a given personality trait it may suffice to know what the *individual believes* he experienced or *believes* about himself even though the belief have either no foundation in fact or a very flimsy one. The phantom baseball practice indulged in by the bed-ridden psychiatric patient is understood as soon as he tells us the soul of Christy Mathewson has taken possession of his body. Similarly, the truculence and negativism of a student with respect to a given class can be understood when he assures us the 'teacher has a grudge' against him, even though independent investigation proves incontestably the fairness of the teacher and reveals the misinterpretation of a specific classroom episode responsible for the student's conviction. This possible objection to the validity of the autobiographical method is accordingly not particularly damaging.

Although the distinction between such a method and that which is ordinarily regarded as the psychoanalytic method *per se* is not ordinarily made either in critical discussions of the movement or in actual psychoanalytic practice, clear thinking, it seems to us, necessitates such a differentiation. Freud has really presented the psychological world with two methodological possibilities: the autobiographical technique for gathering the facts and the psychoanalytic technique as a means of interpreting them. These possibilities call for independent evaluation. Without being inconsistent, the first

might be endorsed and the second rejected. Whether the converse would hold is open to question, for the validity of the psychoanalytic diagnostic devices is a function of the validity of the material to which they are applied. The validity of each must be separately determined. Furthermore, acceptance of the fruitfulness of the autobiographic method does not involve sanctioning of such concepts as that of the unconscious, the Œdipus complex, the id and similar fruits of the psychoanalytic method. In fact, the existence of the schools of Adler and Jung indicates the justification for the independent evaluation of the two methods. Both of these schools endorse the autobiographic method of Freud while they diverge in terms of his psychoanalytic method.

Academic psychology has been largely indifferent to the use of the autobiographic method as a research tool.[5] This is understandable in the light of what has just been said concerning our failure to separate it from the psychoanalytic method. Justifiably skeptical regarding the vagaries and bizarre interpretations produced by the latter technique, it has tended to spurn the whole business. And yet, despite this official rejection, it has quietly absorbed a number of Freudian concepts. Defense mechanisms, rationalizations, projections, complexes and resistances, while alluded to on occasion in different terminology, of course, in pre-Freudian days, have since taken more focal positions in the writing and thinking of scientific psychologists.

It is somewhat strange that a method condemned as unscientific should yield such apparently necessary and fruitful psychological concepts. We believe what has happened here is that the academic psychologist has found indubitable, if casual, introspective and considerable observational support for the existence of mental characteristics which Freud discovered primarily by means of his autobiographical approach. Support of this kind by its cumulative force gradually induced the psychologist to incorporate the facts into his professional thinking while with apparent inconsistency he continued to regard psychoanalysis as at best a pseudo-science. Very

[5] See footnote 2 for some exceptions to this statement.

likely he was not aware of what was going on in his thinking. However, the strange anomaly loses its strangeness and its paradoxical character if the analysis we have made is sound. He was accepting data obtained by the concrete autobiographic phase of the movement, while maintaining a disapproving attitude toward the more speculative, highly symbolic psychoanalytic phase. It may be of interest to point out, in this connection, that even Watson, whose strictures on psychological method amount to a suicidal solipsism, has not been uninfluenced by Freudian contributions. His *Psychological Care of Infant and Child* is replete with admonitions to the mother to avoid the establishment of infantile fixations of a sort that Freud would label incestuous. Watson's words are different, but his music is the same.

With due allowance for exceptions, the picture of mental life, portrayed in our traditional text-books is on the whole vastly different from the picture painted by Freud. Both the psychology of philosophers since the days of Descartes as well as the experimental psychology of the last hundred years can be regarded as predominantly concerned with epistemology. What and how do we know, are the questions back of the rationalism of Descartes, the empiricism of the British associationists, the sensory physiology of Johannes Müller and Helmholtz, and the majority of experimental studies undertaken from the days of Fechner to the present. Sensation, perception, memory, and learning are the fields which have yielded most readily to the experimental attack, with the consequence that psychological texts have tended to center their accounts of mind around these topics. Because such topics are specialized aspects of epistemology it is convenient to characterize this official and traditional psychology as mainly epistemological in its interests. We are fully aware of the existence of other trends, but they fail to dominate psychological progress, being more sporadic and isolated than the epistemological.

Freud's psychology is manifestly different from these epistemological psychologies. Developing apart from the university tradition, it succeeds in becoming a system unique

in outlook and emphasis. Uniqueness of this sort is particularly apt to occur under such circumstances.[6] It will be recalled that Hobbes, Locke, Hume, Hartley, and the two Mills were none of them university professors, and that it is not unlikely the originality of their empirical psychology was due to this circumstance. Free from the traditional intellectual molds of the scholastic mental philosophy of the time, their thinking was less likely to follow the beaten path of the past. What was a 'new' psychology in its day followed as a result —if this interpretation be correct. Like that older 'new' psychology of the empiricists, the present 'new' psychology of the Freudians, contrasted with the psychology of the universities, is mainly a difference in orientation. Mental life as described by Wundt, Brentano, or James seems to be different from what is presumably the same mental life as an object of study when Freud describes it. In the former the orientation is epistemological; in the latter such is not the case. One searches Freud's writings in vain for the familiar discussions of sensation, optical illusions, perception and association. Cross-sections of the eye, diagrams of spinal tracts, the Müller-Lyer figures, the color spindle—all our old psychological friends are absent and not accounted for. And the reason for this, it seems to us, is that these old friends are of service when we are wrestling with the problem of knowing and are of dubious value in other connections. What Freud is wrestling with in his psychology is not epistemology but mental conflict. The autobiographical data furnished him

[6] James should be consulted in connection with this antithesis between the academic and non-academic treatment of novel phenomena. His viewpoint is made clear in the following passage:

"Facts are only for those who have a mental affinity with them. When once they are indisputably ascertained and admitted, the academic and critical minds are by far the best fitted ones to interpret and discuss them,—for surely to pass from mystical to scientific speculations is like passing from lunacy to sanity; but on the other hand if there is anything which human history demonstrates, it is the extreme slowness with which the ordinary academic and critical mind acknowledges facts to exist which present themselves as wild facts, with no stall or pigeon-hole, or as facts which threaten to break up the accepted system. In psychology, physiology, and medicine, wherever a debate between the mystics and the scientifics has been once for all decided, it is the mystics who have usually proved to be right about the *facts*, while the scientifics had the better of it in respect to the theories." (13, pp. 198–199.)

by his patients evidently failed to lay much stress on those aspects of mental life most emphasized by the professional psychologists. Mind as depicted by these for the most part psychologically naïve witnesses was not primarily an affair of adequate stimuli, two-point thresholds, and engrams, but an affair of inner struggle, yearning for the unattainable or forbidden, and a never quiescent series of desires, longings, wishes, tensions, and cravings. It was around this dynamic vortex that Freud oriented his psychology. The wish and not sensation became the unit of mental life. The optative mood took the place of the indicative.[7]

Most educated people are familiar with the general outline of Freud's resulting psychology and there is no need to expound it here. To evaluate it critically and completely would be a rather profitless undertaking considering the tremendous mass of such literature already available. A more useful purpose might be served by considering some rather neglected phases and some apparently misunderstood phases of the work of Freud. In the interests of candor we ought to add that we have been and still are decidedly distrustful of the accuracy and validity of much of Freud's teaching. To regard any commendatory comment made here as due to the blind enthusiasm of a partisan champion, would consequently be in conflict with the facts. What we are fundamentally concerned with in this paper is to have the work of Freud understood both in terms of its own framework of reference and in terms of its historic setting within the history of psychology.

Academic psychologists as a group have not always been fair to Freud.[8] The metaphorical nature of Freud's descriptive vocabulary has turned many psychologists against him. His terms lack the precision and cold abstractness of laboratory psychology. Hollingworth, in particular, has excoriated psychoanalysis on this score. Because its metaphorical terminology is based on analogy he has dubbed the movement *psychoanalogy* and refused to regard its teachings seriously.

[7] Holt (10) has discussed some of the implications for psychology of this Freudian emphasis on the wish.

[8] Consult Bumke (3), Dunlap (4), Hollingworth (8, 9), and Wohlgemuth (19).

This seems to us to be unfair and possibly due to a failure to appreciate the cogency of analogy as well as a failure to appreciate the circumstances responsible for description by metaphor in Freud's psychology.

We might consider these in order and ask ourselves why either description by analogy or argument by analogy should not be accepted as valid. Does not scientific inference involve analogy? In astronomy distortion of a planetary orbit has been attributed to the gravitational influence of a still unknown planet because such distortion is analogous to, not necessarily identical with, such influence in the case of observed planets. The physicist infers or conceives light to be a wave on the analogy of perceived water waves. He explains what he means by his electrical units of volt, ohm, and ampere by invoking the analogy of water coursing through a pipe. Upon what does the biological classification of genera and species rest if not, in part at least, upon morphological analogy? When the physician tells us we have appendicitis because the pain is localized over McBurney's point, the temperature elevated, and the white blood count tremendously increased is not such diagnostic inference a form of analogy?[9] What it amounts to is that in the past he and his predecessors have found such an array of symptoms occurring in organisms *like* ourselves to be correlated with an inflamed appendix when that organ was surgically exposed. The symptoms he discovered in us are sufficiently like those he has previously met with that he completes the analogy by predicting what surgical intervention will reveal. At the diagnostic stage an inflamed appendix is inferred or assumed to exist on an analogical basis and is not perceived. Similarly, when another patient complains that his skin feels as if bugs were crawling over his arm, this analogical description causes the physician to look for evidence of addiction to cocaine. He does not dismiss the complaint as mere analogy. It is true, of course, that analogies may be crude, inaccurate, and misleading, but

[9] A stimulating discussion of the whole problem of the logic of diagnosis and interpretation in medicine is to be found in the essay by Rudolf Allers on *Begriff und Methodik der Deutung* (16, pp. 86–128), in which Husserl's system of logic is applied to some of the problems with which we are concerned in this paragraph.

the same may be true of any inference. Finally, if analogy is the treacherous cognitive instrument Hollingworth seems to think it is, how does it happen that the analogies test occupies so important a place in our intelligence tests? Surely one reason must be that the perception of relevant relationships, which is the essence of analogical thinking, is also a constituent part of intelligent behavior.

Reverting to the second criticism of Freud, the one having to do with the metaphorical nature of his descriptive vocabulary, a word or two of explanation may be in order. It must be remembered that Freud was confronted with the task of describing phases of mental life neglected by official psychology. The phenomena he was observing and inferring had not yet been subjected to the scientific scrutiny of academic psychology. Furthermore, because of the esoteric nature of the phenomena themselves, every-day speech had failed to build up a popular terminology serviceable in the way in which we employ such terms as imagination, memory, and vision. Under the circumstances there was nothing else to do but to invent a descriptive vocabulary. The spontaneous way of disposing of such a problem is to resort to analogy or association by similarity except when we are desirous of honoring a name. In the latter instance we can label the new object—a new baby, or a new instrument, or new plant—after some human being. We use such terms as John Smith, Bunsen burner, or Dorothy Perkins rose. Even many processes, as opposed to entities, receive such description by baptism. Roentgen rays, kilowatt hours, and microfarads are convenient illustrations of this. Description of this sort is utterly arbitrary and the term selected bears no intrinsic relationship to the object or process described. On the other hand, such terms as sclerotic coat, bacillus, and ions receive their descriptive relevance from salient features of the new which are similar to familiar features of the already known. If the three tunics of the eye differ in hardness it is not illogical to label the toughest the hard or sclerotic coat. A bacterial organism that reminds us of the shape of a small stick becomes a bacillus. Similarly electric charges conceived of as travel-

ling over a charged field are compared to human wanderers and called ions. This is description by metaphor and is found in all fields of science. To censure Freud as unscientific because of abundant use of metaphor is consequently unmerited. When he postulates what he designates as a censor or as an Electra complex he is following established scientific precedent. There is no more justification for regarding such Freudian technicalities as naïve anthropomorphisms than there is for accusing the neurologist of believing the mind rests on the thalamus because the word in question means couch or bed. In short, we must bear in mind that our routine vocabulary for describing the complexities of our emotional life is pitifully inadequate. The endeavor to describe affective impulses or volitional conflicts not covered by this routine nomenclature carries with it the need for new terminology.

This was the task confronting Freud. His concept of a psychic censor, for example, is no more mythical or speculative than Spearman's G factor, Watt's mental set, or the negative sensation of Fechner. To argue that Freud conceived of the censor as a little man within the mind who passes muster on communications coming from the unconscious is tantamount to saying the chemist conceived of a cation as a wandering negative spirit seeking its mate, the anion, or wandering positive spirit. Metaphoric language merely indicates the nature of processes assumed to exist in order to account for observed events. The term censor is a process functioning in such a way as to exercise a selective influence on mental content. It is analogous to the work done by a military censor; hence the term. To interpret the analogy as a relation of identity under the circumstances is to forget the *as if* character of the metaphor. It is akin to regarding such a concept as mental set as meaning an actual shift of some psychic screws or thought waves depending on whether we think of the metaphor as referring to a watch or a shock of hair that has to be set. In reality all the term means is the inferred process presumably responsible for the specificity of control involved in regulated associative sequences. Only

the latter is observed. That is to say, we observe the fact of delimited associations of a given kind when instructed to give the antonyms of a list of words, and of another kind when told to respond with synonyms for the same list. But the mechanism or process by means of which this delimitation is brought about is not observed. Its existence is purely inferential and its nature unknown; but we find it necessary to have a name for it. Being able to refer to this perceptually unknown process as 'mental set' facilitates our psychological analyses of memory, association, and reasoning. Future evidence may cause us to scrap the concept just as synaptic resistances have been scrapped, but for the time being it is a useful conceptual and heuristic device. Such familiar psychological terms as temperament, mental act, the self, and neural impulse have pretty much the same status.

In considering the tenability of Freud's psychological concepts, it might be well to bear this in mind and not permit his use of metaphorical labels, many of them derived from Greek mythology, to prejudice us against due evaluation of the mental processes to which they are intended to refer. They are to be rejected either because the facts upon which they are based cannot be verified, or because we can supply better concepts if the facts are verified. To reject them because they are described in metaphorical terms is, assuming the validity of our exposition, a scientifically indefensible procedure.

Academic psychology differs from Freud's, we suggested, because the latter developed independently of the university tradition. Examined more closely this means a difference in point of departure. For Freud this was the problem of mental conflict. Now such conflict resolves itself into the problem of ethical choice or, more accurately expressed, a conduct problem. His view of psychology, in other words, developed on the basis of his study of the mind of man when confronted with personal problems. The genesis of his psychology is in the field of axiology or theory of value. We do not mean that Freud actually envisaged his approach in this fashion but rather that in terms of a larger perspective

it amounts to this. At the start of his work he was primarily interested in getting at the basis of the neurotic personality. He found this basis in the persistence of incompatible wishes. Wishes have to do with desires and desires with what we value. When the selection of one value involves the sacrifice of another we have a conflict, or to put it differently: every vital choice calls for inhibition. How the individual chooses becomes an important determiner of his character and personality. How he disposes of his rejected longings and ambitions becomes a determiner of his mental balance. Indeed such phrases as 'mental balance' and 'integration of personality' are intelligible metaphors only when we recognize the existence of conflicting urges within the mind of man.

Freud, basing his psychology on data obtained from subjects whose handling of such conflicts proved ineffective, came to recognize three sources of the incompatible wishes: a low level of primarily crude primitive urges, the *id*, a middle level of urges acceptable to the self and hence called the *ego*, and finally, the highest level of urges, the *super-ego*, those reflecting our most socialized, ethically mature dynamic ideals. If our analysis of the genesis of this tripartite division is sound, it is a result of approaching the study of mind from the viewpoint of axiology, particularly as exemplified in the sphere of conduct problems.

Academic psychology, as we have indicated, has neglected this approach almost entirely in favor of the epistemological. Now an exception to the latter generalization is to be found in the case of Plato. He too, it will be recalled, more than two thousand years before Freud, found it necessary to examine the nature of mind in order to get an answer to a conduct problem; namely, what constitutes a just act? Plato's orientation is thus primarily axiological. What is of particular interest in this connection is that the system of psychology resulting from this orientation should, in its broad outlines, be so strikingly akin to Freud's. In Plato we find an analogous tripartite division: the rational soul, the noble or higher irrational soul, and the appetitive or baser irrational soul. Furthermore, just as Freud originally had a bipartite division

in terms of conscious and unconscious or reason and libido, so we find a similar initial bipartite division in Plato. Consider the following quotation from *The Republic:*

> Then we shall have reasonable grounds for assuming that these are two principles distinct one from the other, and for giving to that part of the soul with which it reasons the title of the rational principle, and to that part with which it loves and hungers and thirsts, and experiences the flutter of the other desires, the title of the irrational and concupiscent principle, the ally of sundry indulgences and pleasures.[10]

Strikingly enough, even the broad meaning Plato attributes to his concupiscent principle is anticipatory of the wide connotation Freud built up around his concept of libido. Plato's famous figure of the charioteer struggling to master his team of spirited horses as symbolic of the mind of man might serve equally well to exemplify the psychology of Freud. By way of facilitating appreciation of this parallelism we shall introduce Rogers' (**15**, p. 80) version of this figure. Referring to the winged horses of Plato, Rogers says:

> One of these is of noble origin, and the other of ignoble; and so naturally there is a great deal of trouble in managing them. The noble element is striving continually to mount to the region of the heavens, where it may look upon the images of divine beauty and wisdom that are proper to its nature, but the body is ever dragging it down to the earth and earthly delights. Now just as, in the state, justice consists in the proper subordination of the different classes, so the just soul is one in which a similar subordination of parts exists, where the charioteer has got control of his steeds, and can guide them to the heights of heaven, where the body submits itself to the sway of the soul, and the beast in man to that in him which is truly human.

The essentially modern concept of personality integration with its ethico-social implications is brilliantly symbolized by this Platonic figure. It indicates most effectively the need for earnest and systematic consideration of the ethical phase of conduct problems by the psychiatrist and practicing psychoanalyst. Such a current mental hygiene shibboleth as 'ad-

[10] Quoted from p. 34 of Rand's Classical Psychologists.

justing to reality' is almost unintelligible when critically examined. We are not referring to any metaphysical difficulty with the word *reality*, but to a more pragmatic difficulty. Almost any instance of personal and significant mental conflict will illustrate the difficulty.

Suppose an able-bodied conscientious objector—to avoid partisan complications, let us make him a Japanese scholar—to be in such distress that he seeks a mental hygienist for aid. The salient facts are easily established. His country is at war and the populace is enlisting with patriotic enthusiasm, while he, in terms of his ethico-social standards, finds himself unable to support war either directly or indirectly. There are two realities here: the flag waving slacker-hating reality of his militaristic environment and his inner reality of the stupidity, sinfulness, and futility of war and the blessings of peace. What positive aid is rendered such a patient by telling him to adjust to reality? And if the psychiatrist ventures to be arbitrary and select one at the expense of the other will that automatically dispose of the conflict? In every case of such mental conflict we have not one reality, but two or more. To make the psychiatric ideal 'adjustment to reality' is either question-begging jargon or a bit of vacuous tautology tantamount to telling the patient to find out the nature of his difficulties and then solve them.

Now it is the particular merit of Plato's attack on the problem of mental conflict that he saw the need for articulating the psychological with the ethical. Personality integration is thus a function of the hierarchy of values by which the individual chooses to live. Freud's investigation of the mental background of those who have failed to achieve such integration lends confirmation to this Platonic insight. It would carry us too far afield to develop the full implication of this here. For our purposes it must suffice to note how both Plato and Freud coming to psychology via axiology find fundamentally the same dynamic pattern as characteristic of mind.[11]

[11] Some weeks after completing this article I found that Swift (18, p. 260) has also noted the parallelism between Plato and Freud. My colleague, Dr. Carlos Kling, was kind enough to call my attention to the fact that Durant (5, p. 33) has also antici-

In conclusion, let us mobilize the essential items of this somewhat discursive appraisal. After first pointing out Freud's emphasis upon psychoanalysis as a *psychological* system, in contradistinction to a psychotherapeutic system, we called attention to the two methodological techniques he has introduced: the autobiographical and the psychoanalytic. We then proceeded to account for the divergence of his system from systems of academic psychology in terms of the epistemological nature of the latter as contrasted with the axiological nature of the former. Charges made against Freudian psychology because of its abundant employment of analogy and its frequent use of description by metaphor were next considered. In this connection we ventured to justify such procedures as involving no breach of accepted scientific method, since both the use of analogy and description by metaphor can be demonstrated to be legitimate scientific tools. Finally, the broader aspects of Freud's psychology were brought into relation with the history of psychology by a consideration of the parallelism revealed by a comparison of the dynamism of his system with that of Plato. This parallelism was attributed to the fact that both Plato and Freud needed an account of mental life that would articulate with the facts of mental

pated me in noting the existence of the parallelism. In fact, Durant's treatment of the relationship is so striking as to merit the following explicit quotation:

"Plato passes on to a remarkable anticipation of 'psychoanalysis.' Our political psychology is perplexed, he argues, because we have not adequately studied the various appetites or instincts of man. Dreams may give us a clue to some of the subtle and more elusive of these dispositions.

"'Certain of the unnecessary pleasures and instincts are deemed to be unlawful; every man appears to have them, but in some persons they are subjected to the control of law and reason ['sublimated'], and the better desires prevailing over them, they are either wholly suppressed, or reduced in strength and number; while in other persons these desires are stronger and more abundant. I mean particularly those desires which are awake when the reasoning and taming and ruling power ['censor'] of the personality is asleep; the wild beast in our nature, gorged with meat and drink, starts up and walks about naked, and surfeits at his will; and there is no conceivable folly or crime, however shameless or unnatural—not excepting incest or parricide ['Oedipus complex']—of which such a nature may not be guilty. . . .'"

That Freud himself noted some of these Platonic parallels appears evident from the work of Jastrow (12, p. 59) which was called to my attention after the completion of this paper. According to Jastrow, "Freud cites from Plato's 'Republic' that the virtuous man 'contents himself with dreaming that which the wicked man does in actual life.'"

conflict. In both systems we find psychology oriented with reference to axiology and this, in our opinion, is what accounts for the cleavage between their systems and those of the academic tradition. In a word, it is the cleavage between axiology and epistemology.

REFERENCES

1. Allport, G. W., and Vernon, P. E., Studies in expressive movement, New York, The Macmillan Co., 1933, xiii + 269.
2. Bühler, C., Der menschliche Lebenslauf als psychologisches Problem, Leipzig, S. Hirzel, 1933, xvi + 328.
3. Bumke, O., Die Psychoanalyse, Berlin, Julius Springer, 1931, pp. 75.
4. Dunlap, K., Mysticism, Freudianism, and scientific psychology, St. Louis, The C. V. Mosby Co., 1920, pp. 173.
5. Durant, W., The story of philosophy, New York, Simon and Schuster, Inc., 1926, xiii + 592.
6. Freud, S., Article on psychoanalysis, Encyclopaedia Brittanica, Vol. 18, 14th Edition.
7. Hartmann, H., Die Grundlagen der Psychoanalyse, Leipzig, Georg Thieme, 1927, pp. 192.
8. Hollingworth, H. L., The psychology of thought, New York, D. Appleton and Co., 1926, xvii + 329.
9. ——, Abnormal psychology, New York, Ronald Press Co., 1930, xi + 590.
10. Holt, E. B., The Freudian wish and its place in ethics, New York, Henry Holt and Co., 1916, vii + 212.
11. James, W., The varieties of religious experience, London, Longmans, Green and Co., 1902, xii + 534.
12. Jastrow, J., The house that Freud built, New York, Greenberg, 1932, ix + 297.
13. Kallen, H. M., The philosophy of William James drawn from his own works, New York, The Modern Library, 1925, vii + 375.
14. Murphy, G., and Murphy, L. B., Experimental social psychology, New York and London, Harper and Brothers, 1931, ix + 709.
15. Rogers, A. K., A student's history of philosophy, New York, The Macmillan Co., 1915, xiii + 511.
16. Schwarz, O., (Editor), Psychogenese und Psychotherapie körperliche Symptome, Vienna, Julius Springer, 1925, xviii + 481.
17. Starbuck, E. D., The psychology of religion, London, Walter Scott, 1901, xx + 423.
18. Swift, E. J., The jungle of the mind, New York, Charles Scribner's Sons, 1931, xi + 340.
19. Wohlgemuth, A., A critical examination of psychoanalysis, London, George Allen and Unwin, Ltd., 1923, pp. 250.

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AN EXAMINATION OF CONDITIONED REFLEX THEORY

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Progress is made in any branch of science not only as new findings accumulate but also as these are interpreted and evaluated. The studies of conditioned reflexes have furnished psychology with many new findings, but as yet there has been no agreement as to the significance of these studies. For some psychologists the conditioned reflex is merely a laboratory artifact with practically no general significance, while for others it is the fundamental form of learning. To the author it is neither artifact nor fundamental form of learning, but rather it reveals or contains the more important principles of learning. It is in defence of this view that this paper is written.

The over-simplification found in textbooks should not mislead the advance student, but evidently the textbook descriptions of conditioned reflexes have been taken as accurate, for in the technical literature there is much material which assumes that a conditioned reflex is what the textbooks say it is. Those who consider the conditioned reflex as a laboratory artifact have made only a slight mistake: the conditioned reflex to which they refer is a textbook rather than a laboratory artifact and so, too, is the conditioned reflex which is considered the fundamental form of learning. In justification of these statements it is necessary to re-examine the experimental studies to discover the nature of conditioned reflexes and the circumstances under which they are developed.

The salivation produced in Pavlov's dogs is not a reflex in the physiological sense of a simple isolated response to a simple stimulus. It is quite different from the knee-jerk which is more nearly the whole response to a blow on the tendon. The salivation is only a part of the whole reaction and a more or

less adequate index of the whole. Pavlov considered it as an index adequate for the study of the problems in which he was immediately interested. But since salivation in response to food in the mouth has been called a reflex and since in textbooks it is written as the whole response to food in the mouth, some psychologists are evidently surprised to find in their studies that the response to a substitute stimulus is different from the response to the original stimulation.[1] There is, however, nothing in Pavlov's studies to show that the conditioned response is the same as the unconditioned one. In fact, it borders on the absurd to assume that the total response to a bell is the same as the total response to food in the mouth; and when only the salivation is considered, it is to be remembered that Pavlov found that the saliva secreted in response to a substitute stimulus differed from that secreted in response to the unconditioned stimulus. If the Pavlovian studies are taken, as they usually are, as indicating most accurately the nature of a conditioned reflex, then the conditioned reflex must be defined as a response which only resembles (has elements in common with or approximates the form of) the unconditioned one.

If the complexity of response made by Pavlov's dogs has been neglected in both textbook and periodical discussions, so even to a greater extent has the complexity of stimulation been disregarded. In schematic diagrams and in discussions only the so-called unconditioned stimulus is included, but this stimulus is far from being the adequate or total cause of the response. Rather it is only the immediate cause or trip-stimulus. The dog secretes saliva when he is given food *and* when he is hungry *and* when he is in a situation similar to those in which he customarily eats. To obtain full salivation and conditioning, there must be (1) an unconditioned or trip-stimulus (food to be eaten), (2) an internal state or stimulus, a motivation, set, drive, desire, craving, (hunger), and (3)

[1] E. G. Wever, The upper limit of hearing in the cat, *J. Comp. Psychol.*, 1930, 10, 221–234; M. Upton, The auditory sensitivity of guinea pigs, *Amer., J. Psychol.*, 1929, 41, 412–421; E. R. Hilgard, Conditioned eyelid reaction to a light stimulus based on the reflex wink to a sound, *Psychol. Monog.*, 1931, 41, No. 184; K. A. Williams, The conditioned reflex and the sign function in learning, PSYCHOL. REV., 1929, 36, 481–497.

reinforcing or contributory stimuli, (stimuli which fit in with or are associated with eating). The effectiveness of the trip-stimulus is dependent upon internal condition and external circumstances, not relatively independent of these as is the simple stimulus for a simple reflex such as the knee-jerk. This dependence upon internal condition was implicitly recognized by Pavlov in his use of hungry dogs, and time and again studies from his laboratory have shown the effect of the general situation. In fact, in the early stages of training, there is no more reason to speak of the bell as the conditioned stimulus than there is to say that the harnessing of the dog or placing him on the table is the conditioned stimulus. What really occurs is that the dog becomes accustomed to eating after a complex series of stimulations, and all the elements of the series become conditioned stimuli to a greater or lesser extent, the extent depending upon the regularity with which they are a part of the feeding situation and the constancy of the interval between them and the actual feeding. Since in actual practice the bell is quite regular in both respects, the salivation becomes highly conditioned to it.

This disregard of the role played by the internal state and by reinforcing stimuli has led psychologists interested in motivated learning to hold that the conditioned reflex bears little resemblance to the learning which they study. Instead of discarding conditioned reflex studies as valueless, the motivationists might well have made more of the point that without motivation there is no learning, not even a conditioned reflex.

This emphasis upon the trip-stimulus alone is clearly revealed in the supposition that all that is necessary in building up a conditioned response is the repeated presentation of a conditioned and an unconditioned stimulus. This supposition is inherent in the conventional statement of the requirements for establishing a conditioned response, and it has led those who attack the frequency theory of learning to believe that they were attacking conditioned reflex theory.[2] Frequent

[2] Tolman and Thorndike are notable examples. Thorndike concludes that his data showing the inadequacy of frequency make it unnecessary to give conditioned reflex theory further consideration.

presentation of food and a bell to a dog will not establish a conditioned reflex in the dog any more than would presentation of similar stimuli to a table establish a conditioned response in the table. To establish a conditioned salivation, salivation must take place, and more than that, it must take place as a part of the dog's dominant activity. Salivation can be made to occur by putting acid in the dog's mouth, but a bell rung before the acid is given does not become a conditioned stimulus for salivation, or it becomes a conditioned stimulus only after a great deal of training, according to Pavlov's studies. But when salivation is made to occur by feeding a hungry dog in his customary surroundings and a bell is rung before the feeding, the bell readily becomes a conditioned stimulus. Salivation in one case is a part of the dominant activity while in the other case it is incidental. A frequent sequence of stimulus and dominant response does result in learning, as the writer has attempted to show elsewhere,[3] but the frequent sequence of stimulus and response or the frequent pairing of secondary and unconditioned stimulus does not result in conditioning. If the things which are frequently paired were specified, nine-tenths of the discussion as to the importance of frequency in learning would be pointless.

Up to this point we have been concerned with showing that the unconditioned response is not simple from the standpoint of either stimulus or response, that the conditioned response only resembles the unconditioned one, and that the development of the conditioned response involves a sequence of secondary stimulus and dominant response rather than a pairing of secondary and trip-stimulus.

Our next concern is to examine the factors which affect the stability of the conditioned response once it has been developed. In this connection the findings usually subsumed under the term experimental extinction are immediately encountered. This destruction of the effectiveness of the conditioned stimulus by presenting it alone has usually been

[3] C. N. Rexroad, Outline of the conditions under which learning occurs, PSYCHOL. REV., 1932, 39, 174–183.

considered as a phenomenon belonging solely to the conditioned reflex and as indicating the instability of the conditioned reflex in contrast to the relative stability of much human learning. Just why experimental extinction should have been regarded in this manner is difficult to see. The conditioned stimulus loses its effectiveness exactly as the tuning fork lost its effectiveness for causing the spider to leave its web in the experiments by the Peckhams and much as an object approaching the eye loses its effectiveness for causing one to blink and as a radio loses its effectiveness for causing one to listen to a broadcast. Experimental extinction rather than being a unique phenomenon is a typical case of negative adaptation and comes under the rule which the writer derived in a previous article,[4] the rule that a stimulus loses its effectiveness for a given response when that response does not occur in a dominant manner upon application of the stimulus or when that response is superseded by an incompatible one. The bell (or other conditioned stimulus), when repeatedly given by itself, loses its effectiveness for salivation because it alone cannot produce a salivation which is a part of the dominant activity of the dog. It cannot produce a dominant salivation for three reasons: (1) the effectiveness of the conditioned stimulus is subject to counteraction or inhibition by other stimuli, (2) the conditioned response does not satisfy or terminate the drive which was involved in its development, and (3) the conditioned response is to some extent an anticipatory or readiness response, a type of response which cannot be maintained if the external stimulations which are anticipated fail to appear.[5]

The influence of inhibitors has been clearly and unmistakably shown in the Pavlovian studies, but since in the studies more strictly concerned with experimental extinction these inhibiting stimuli were eliminated as fully as possible, their influence in destroying the effectiveness of the conditioned stimulus is probably insignificant. Yet one cannot deal with animals in vacuo, and so even under the most carefully con-

[4] C. N. Rexroad, PSYCHOL. REV., 1932, 39, 174–183.

[5] Reasons 2 and 3 relate to factors ordinarily stated as the law of effect.

trolled laboratory conditions, one must concede the possibility of counteracting stimuli, especially since the influence is so marked when they are intentionally introduced.

The second reason that the bell given alone loses its effectiveness is that it does not remove hunger. In terms of trial and error theory the conditioned response to a bell is as much an error as going into a blind alley. The hunger continues no matter how vigorously the dog responds to the bell, and soon the hunger stimulation will initiate some activity other than that evoked by the bell, *i.e.* hunger will inhibit the conditioned response even if there are no external inhibitors. Furthermore, the conditioned response itself is in part an intensification of hunger rather than a means of eliminating hunger as is the unconditioned response to food in the mouth. While the bell was gaining effectiveness for salivation, it was also becoming effective for starting stomach contractions, which, if we are to believe the reports of several investigators, are the hunger or the correlates of the hunger. Since the conditioned response intensifies hunger rather than assuages it, this response will be dropped in favor of some other activity, much as scratching at a crack by a dog in a puzzle box will be dropped in favor of some other activity. Just what causes a motivated animal to discontinue one form of activity and take up (try) another until the motive is satisfied is not our immediate concern. The point is that the same factors which cause this shift in activity in any case of trial and error learning cause the conditioned response to be discontinued and finally dropped. If this reasoning is sound, then a conditioned response which is made to satisfy some motive should be continued even though the unconditioned stimulus is never again given with the conditioned one. Culler has shown continuation or stability of the conditioned response under these conditions. After he had built up according to the manner employed by Bechtereff a conditioned leg flexion in a dog, he fed or watered the dog whenever the conditioned stimulus was given. The conditioned response, instead of satisfying the motive involved in its development, satisfied another drive, and under these conditions it remained stable

even though the unconditioned stimulus was never repeated. Likewise much of human learning, whether it be the learning of the nonsense syllables in the laboratory, of the multiplication of tables in school, or safety habits in crossing a street, remains stable because it serves in one way or another to satisfy the individual's desires. The conditioned salivary response is built upon a very specific drive, while much of human learning is built upon more obscure and generalized desires. It is easy to remove experimentally the motive upon which the conditioned salivation is built, and it is, therefore, easy to demonstrate the destructive or extinguishing effect of the removal of a motive upon learned behavior.

Not only does the conditioned response to a bell fail to satisfy the motive involved in its development, but when food is not given, the conditioned response also fails to anticipate correctly the sequence of external stimulation, and in this fact we have our third reason for the extinction of the conditioned salivation. The same conditioning which made the bell effective for salivation and for stomach contractions made it also effective for a postural or muscular readiness to secure and eat food. When food appears, this readiness passes over into a series of recurrent and changing muscle actions, but when food fails to appear, the posture disintegrates, for no single muscular response can be maintained for any length of time. One can fixate a patch of light for only a few minutes, but one cannot fixate even this long a spot where he expects a light to appear, especially if the light fails to appear at the anticipated time. Any prolonged activity involves recurrent and changing muscular contractions, the changes occurring as new external conditions are encountered. When the external conditions upon which the changes are dependent fail to occur, the activity is disrupted. The muscular responses made to the bell are the first responses in the whole activity of eating, are a readiness for eating, an expectation that food will follow, *i.e.*, conditioning is in part teaching to expect or to be ready, and the conditioned stimulus is in part a signal for what is to come. If after the expectations are taught, conditions are so altered that the expectations cannot be realized,

the animal ceases to anticipate the conditions which prevailed during training. Learning would scarcely be learning if animals were incapable of modifying, revising, and discarding their old expectations on the basis of new experiences. The promptness with which an old expectation is modified or dropped is as good a measure of learning ability as is the promptness with which the original expectation is developed. We go to the telephone expecting some one to answer when we speak, and we would soon drop this response if no one ever answered. Most expectations which we build up continue to be confirmed, and in consequence much of our conditioning is stable, but occasionally conditions are so altered that our old expectations must be discarded. If, after a subject learns a series of nonsense syllables by the paired-associates or anticipation method, the order of pairing is changed, the experimenter finds that the subject's original responses have been experimentally extinguished. It makes little difference whether he calls this phenomenon associative inhibition or experimental extinction of conditioned responses. He may, of course, continue his study by requesting the subject to learn both lists. In this case the subject acquires conditional expectations, the expectation that wak will be followed by zut if the list is the first one and that wak will be followed by pam if the list is the second one. Many human responses are of this conditional order. They are developed beyond the stage in which we expect a given stimulus to be followed by one and only one other specific stimulus: they are developed to the stage in which we expect a given stimulus to be followed by any one of several others. In this more developed stage the likelihood that the expectations will not be confirmed is decreased, and consequently our acquired responses are relatively stable.

The line of argument given as our third reason for experimental extinction is approximated by Tolman when he develops the concept of manipulanda and the concept of sign-gestalt-expectations or readinesses,[6] but for him the condi-

[6] E. C. Tolman, Purposive behavior in animals and man, New York, The Century Co., 1932, vii + 463.

tioned reflex is not representative of learning while for me it is typical. And strangely enough it is typical for exactly the reasons it has been held to be non-typical. A conditioned response is not merely one which is evoked by a stimulus originally incapable of evoking it. It is also a response which only resembles the original one, which is developed under motivation, and which is stable as long as the motivation is constant and as long as it is appropriate to the sequence of stimulation which follows it. The conditioned response would indeed be a unique phenomenon if the conditioned stimulus could become equivalent in all respects to the unconditioned stimulus, if the sound of a bell could take the place of food not only in evoking eating but also in satisfying hunger, and it would have a further claim to uniqueness if the response were maintained without contributing to the satisfaction of any motive and in spite of changed experience with stimulation sequences. But an examination of the conditioned response robs it of its claims to uniqueness. In its first aspects it shows a stimulus gaining effectiveness for a given response and reveals the role of motivation and of general situation for this gain in effectiveness. This points to the generalization that a stimulus gains effectiveness when it precedes a dominant response and that motivating stimuli, reinforcing stimuli, and trip-stimuli are necessary for evoking a dominant response. In a second aspect it shows a stimulus losing effectiveness during training. The fact that the conditioned stimulus loses power to evoke its own response as it gains effectiveness for the response tripped off by the unconditioned stimulus is often disregarded. The bell, as it gains effectiveness for salivation, loses ability to cause the dog to prick up his ears and turn toward it, or even more strikingly, the electric shock, as it gains effectiveness for salivation, ceases to produce flinching and withdrawal. This points to the generalization that when a stimulus is followed by a response more dominant than its own, it loses effectiveness for its own response. In its third aspect the conditioned reflex shows a stimulus losing effectiveness after training. In this case the response which has been ascendant during train-

ing is no longer elicited as a dominant response. Its dominance is destroyed (1) by replacing the reinforcing stimuli with inhibiting stimuli, (2) by discontinuing the satisfaction of a motive by means of the response, or (3) by changing the sequence of external stimulation from the sequence which prevailed during training. This points to the generalization that when a stimulus fails to be followed by a dominant response, it loses what effectiveness it had, or when a stimulus is followed by a variety of dominant responses, it loses effectiveness for its old response without gaining sufficient effectiveness for any other one response to be capable of evoking it. In its first aspect the conditioned reflex is substitution of stimulus, in its second it is substitution of response, and in its third it is negative adaptation. In no aspect is there a simple pairing of stimuli which accounts for the changes displayed, but rather in all aspects motivation and the satisfaction of motives are involved. In this respect the conditioned reflex and trial and error learning come together.

If this examination of the conditioned reflex is accurate, two conclusions are to be made. (1) The strictures leveled against conditioned reflex theory are justified if the conventional interpretation of the conditioned reflex is accepted, but they are pointless if the Pavlovian studies are taken as a standard. (2) The conditioned reflex is not a form of learning to which all other forms reduce, but rather in its different aspects it is all forms of learning, or better still, it reveals the primary and secondary factors involved in all learning.

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PROXIMODISTAL DIFFERENTIATION OF LIMBS IN YOUNG ORGANISMS

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In a series of articles (**13**, **14**, **15**) in this journal an attempt was made to gather and evaluate the evidence, both morphological and behavioral, for the organismic hypothesis. On the basis of this approach some recommendations were made for the purpose of promoting experimentation in the field of infant reactions. One suggestion concerned the advisability of studying the events occurring in the proximal region of the arms of infants while the reaching and grasping patterns reported by Halverson (**9**) were developing. It was suggested that these events illustrate the differentiation of a specific pattern from generalized or mass responses, and that some photographic observations of our own on infants indicate a progress of differentiation down the limb culminating in grasping. In other words, the grasping patterns studied by Halverson constitute a final stage of a series of stages which is analogous to the anatomical principle of proximodistal development of the limbs. McGraw (**20**) has questioned the validity of this suggestion, and also accuses the writer of an unscientific enthusiasm for a pet theory.

In many controversies the difficulty arises because the two parties talk about different matters which, for the moment, seem to be identical. In the present instance this seems to be the case. McGraw and the writer are discussing reaction patterns which in time at least are distinct. She is concerned with the grasping of newborns when they are suspended on a stick. This response, like the Babinski, opisthotonus, Moro's reflex, etc., drops out after a time and later during the first year of life becomes organized into what some would call a voluntary response, the grasping which occurs during prehensile activity. In the interest of clarity, the latter might

be named the 'Halverson response' since it was analyzed by him (9). The early grasping response noted by many observers and studied recently by Chaney and McGraw (4) should be called 'suspended grasping.' It is obvious that McGraw's remarks concern suspended grasping, whereas the writer was concerned in his original recommendations with the Halverson response.

However, it is still a fair question to ask whether suspended grasping, likewise, is an instance of proximodistal differentiation. McGraw (20) holds that it is not on the grounds (1) that the proximodistal principle does not hold for the embryological development of the limbs, and (2) that grasping behavior first differentiates in the digits and not in the shoulders and upper arms.

In regard to the proximodistal growth of embryonic structures, McGraw says, "Had Irwin considered the embryological development of the limb buds he would have realized that differentiation of the limb buds and concurrently their innervation is initiated not in the shoulders and upper arms but in the digits. Embryologically, the development of the hands precedes the development of the upper arms" (20, p. 301). Again ". . . in the extremities both nerve and muscle differentiation first appears in the distal end" (20, p. 302). The assumption here plainly is that structural differentiation of distal parts of the arm precedes that of proximal parts.

McGraw in her note fails to quote or refer directly to specific embryological studies. Since she grounds her contention in the manner of muscle and nerve differentiation, the first part of the reply will include a summary of findings on the development of these structures. Later, evidence on the behavior side will be presented.

Arey (2, p. 331) after describing the undifferentiated mesenchyme of the limb bud says, "From this diffuse tissue, which at about 9 mm. forms condensed premuscle masses, the limb muscles are differentiated, the proximal ones being the first to appear. The progressive modelling of distant muscles reaches the level of the hand and foot in embryos of seven weeks. The upper limbs naturally maintain an ad-

vance over the lower throughout development." Keibel and Mall (17, Vol. 1, p. 548) likewise hold to the proximodistal principle. "In both arm and leg the proximal muscles are the first ones to be recognized. There is a progressive proximo-distal differentiation; the hand and foot muscles being the last to differentiate. The muscles of the arm differentiate slightly earlier than those of the leg."

Bardeen and Lewis (3) and Lewis (19) are the investigators whose studies have cleared up the embryological growth of human limb structures. Using a series of human embryos between the ages of two and seven weeks, they reported on the external and internal aspects of differentiation of the limbs. They include specific observations on the direction of limb development. Lewis (19, p. 155), in his study on the development of the arm in man, states, "the premuscle masses most clearly associated with the trunk are the farthest advanced, those connecting the arm and trunk next, and the least developed is the general arm premuscle sheath, especially its distal portion." Bardeen and Lewis (3, p. 35) report, "The structures of the upper arm and thigh are differentiated before those of the forearm and leg, and the latter before the hand and foot."

The embryological work on the development of the nerves of the limbs consistently shows the same direction of differentiation. This work has been done using both infrahuman and human materials.

Harrison (10) performed a fundamental experiment on the manner of outgrowth of nerves of Rana esculenta. "The axis cylinder of the nerve fiber is the outgrowth of a single ganglion cell, with which it remains in continuity throughout life. It grows gradually from the center towards the periphery establishing secondarily connection with its end organ" (10, p. 131). This study was followed by a series of experiments by Harrison (11, 12) and by Detwiler (7, 8). The latter, using the limb bud transplantation technique, studied the formations of nerve plexuses and the outgrowth of peripheral nerves in Amblystoma. His results corroborate those of Harrison.

The work of Weiss (**23**) on the transplantation of fully developed limbs of the larvae of Salamander maculosa further supports these results. In regard to the regeneration of nerves under these conditions, he says, "When a limb is transplanted its nerve paths are interrupted; at the cut end the severed nerve fibers divide into sprouting branches which, multiplying and growing out toward the periphery, invade their own old paths and also the degenerated nerve paths in the transplanted limb" (**23**, p. 241). Coghill (**6**) likewise indicates the invasion of the limb by branches of nerves which originally innervate the muscle segment. Thus, it seems to be well established that in infrahuman materials peripheral nerves grow out distally through the limbs.

In regard to the development of limb nerves of human embryos a similar differentiation has been demonstrated. Keibel and Mall (**17**, Vol. 2, p. 122) say, "When the limb buds first form they consist, to all appearances, of homogeneous mesenchyma and contain no nerves. Very soon, however, opposite the base of each limb bud, presumably stimulated by its presence, the anterior primary divisions of the spinal nerves undergo an exuberant growth and form a solid sheet or plexus of fibers extending toward the base of the limb. In embryos a few days older, coincident with the condensation of the skeletal core, branches from this nerve plexus can be seen advancing into the limb bud and entering the areas of premuscle tissue that in the meantime have formed a sheath around the skeletal core. The premuscle sheath is not evenly distributed, but from the very start is arranged in the form of muscle groups, and it is between these groups through the intermuscular spaces that the nerves make their way. As the differentiations of the limb continue the nerve trunks extend distalward in the limb and give off muscle branches which enter the muscle groups and supply the individual anlages." Bardeen and Lewis (**3**, p. 15) report that, "The brachial plexus is formed and nerves grow into the anterior limb during the latter half of the fourth week. The lumbosacral plexus is formed, and nerves grow into the posterior limb during the first half of the fifth week."

These studies show that the progress of nerve development in the limbs, like that of muscular development, proceeds from the trunk distalward.

Lewis (**19**, p. 181–182) has summarized the development of the human arm structures in the following: "The first indications of the arm bud appear during the third week as a slight swelling in the lower cervical region on the anterior portion of the Wolffian ridge. This gradually enlarges and by the time the embryo is 4.5 mm. in length and three weeks old the arm is of considerable size. The base now lies opposite the lower four cervical and first thoracic vertebrae. The arm bud is at first filled with a homogeneous and closely packed mesenchyma. No nerves or myotome buds have entered the arm, yet it contains the tissue from which the muscular and skeletal elements develop.

"During the fourth week, before the nerves enter, differentiation begins by an increased condensation for the skeletal core. The nerves, however, have reached the base of the arm and have united by their expanded ends into the first beginnings of the cervicobrachial plexus. During the fifth week the nerves from the plexus push into the premuscle sheath which surrounds the skeletal core.

"By the end of the fifth week the skeletal core can be differentiated into many of the skeletal elements, three of which contain cartilage, namely, the humerus, ulna, and radius. The premuscle sheath also has become more or less differentiated into muscles or groups of muscles, between which, however, no sharp lines can, as a rule, be drawn. Toward the distal end the differentiation is less complete, and in the hand premuscle tissue still represents the intrinsic muscles. The nerves have grown into the hand and spread out in a very peculiar manner. Most of the branches of the brachial plexus found in the adult are now present.

"By the end of the sixth week most of the muscles of the arm are easily recognized. The intrinsic muscles of the hand are just beginning to show fibrillation and are still mostly of premuscle tissue."

It is quite apparent that McGraw's (**20**, p. 301) statement,

"differentiation of the limb buds and concurrently their innervation is initiated not in the shoulders and upper arms but in the digits," has little support from embryology.

The second criticism is based on the development of behavior patterns themselves rather than on anatomical and embryological considerations. "If Irwin is to apply this analogy with respect to the function of grasping, and other types of behavior involving the extremities, he should expect differentiation to occur first in the digits and not in the upper arms. It is the writer's observation that such is the case" (**20**, p. 301). The assumption here is that behavior differentiation begins first in the distal rather than in the proximal region of the arm. This assumption can be tested in the light of work already completed on the development of limb behavior.

Coghill (**5**, p. 128) reported the development of limb movement in amphibia. "It is found that limb movements appear in the following order: limb movement integrated with trunk movement, discrete movement of the limb as a whole, forearm movement integrated with movement of the limb as a whole, discrete forearm movement, hand and digital movement integrated with forearm or with arm as a whole. The same order holds for the development of movements of the hind limb." In regard to the various flexion movements of the arm, Coghill (**6**, p. 20) has this to say, "The first elbow flexion occurs with action of the arm as a whole, and the forearm later on acquires the independence of a local reflex. So also it is with the movement of the hand and digits." Thus, Coghill's work on the behavior of limb activity of Amblystoma punctatum neatly illustrates the development of specific reactions out of mass activity, the progression occurring from proximal to distal parts.

Kuo (**18**, p. 415) has made some observations on development of behavior in the chick embryo which is relevant to the matter. "It must be noted in passing that separate motions of the toes, though not independent of the motions of the other parts of the leg, are observed in some cases on the seventh day. It must be recalled that the toes in the sixth and

seventh day are rudimentary and morphologically not separate. On the eighth day the toes can move apart from each other by 2 or 3 mm."

There are some valuable studies on mammalian embryonic behavior which confirm the proximodistal principle. Angulo y González (1) has worked on the development of behavior of the white rat. He says, "Motility first appears in albino rats at about fifteen days and eighteen hours after insemination. In older fetuses this motility spreads in the cephalocaudal and proximodistal direction" (1, p. 442).

By far the most convincing evidence that grasping is a final stage of proximodistal behavior differentiation has been furnished by Windle and Griffin (24) in their work with cat fetuses. "Foreleg motility appeared first as part of the trunk wave of contractility, and the fact that the proximal musculature seems to be involved before the distal may be related to this. With the exception of the early, apparently nonnervous, 'passive' phenomenon, the first active flexion did not make its appearance before 18.5 mm., and the presence of it here is not beyond question. It was definitely present at 19.5 mm. as a reaction of the limb as a whole (proximal response). At 21 to 22 mm. the elbow was involved, at 27 to 28 mm. wrist bending was observed, at 38 mm. definite adduction of the limb became manifested, *but not until 75 to 80 mm. was definite, digital flexion recorded.*[1]

"The development of motion in hind legs is similar to that of the fore, but occurs at a later time. The earliest flexion of the limb as a whole was a proximal movement and did not appear in specimens smaller than 26 mm. Its spread distally was somewhat slower than in that of the forelimb and movement at the knee was not apparent until the 36 mm. stage; at the ankle joint no positive flexion was seen before 60 mm. Adduction of the limb and flexion of the digits *occurred in those 95 mm. long*[2] and probably somewhat earlier" (24, p. 173). Since flexion of the digits constitutes the basis of the grasping reaction, here is excellent evidence from the develop-

[1] Italics are not used in the original.

[2] Italics are not used in the original.

ment of behavior in one of the mammals that grasping is a product of proximodistal limb differentiation.

In the literature there are no specific observations on the development of human grasping during the uterine period. This is due no doubt to technical difficulties in studying prenatal behavior in the human. Some observations by Minkowski (**21**) on the nature of limb movements of a series of human embryos indicate that at two months the movements of hands and fingers had attained to some degree of specificity, but these movements were of an athetoid-choreic nature suggesting the remnants of mass activity in the hands. Suspended grasping in embryos, however, was not specifically studied by Minkowski. In spite of the scarcity of direct observations on human materials, the fact that the proximodistal progression of limb behavior has been found throughout the vertebrate scale with amphibians, birds, and the lower mammals suggests the probability, at least, of its existence in the human.

It would seem that the evidence in regard to the differentiation of vertebrate limbs, including morphological structure as well as behavior, is convincingly in favor of the proximodistal principle.

McGraw (**20**, p. 301) says, "As a matter of fact the grasping of a newborn infant is localized essentially in the distal phalanges. In comparison to the digital grasp the rest of the body, even the upper arms, seems relatively inactive when the newborn infant holds his body suspended." It appears that this statement tacitly assumes the localization, during the course of differentiation of the grasping reaction, as such, in the proximal parts of the arm and that this pattern, as such, works out distally to the phalanges. If this is the meaning of the statement, then it is based on a misunderstanding of the nature of the differentiation process. This is a case of the 'little man in the egg' fallacy which has been dealt with elsewhere (**16**).

It should be kept in mind that the present discussion is limited to the development of the suspended grasping reaction. Previously the proximodistal view of the Halverson response

was set forth (**14**). There remains the question of the type of neural correlations of these two different patterns. Here it can only briefly be suggested that in terms of the radial hypothesis of differentiation suspended grasping is related to the proximodistal development of the peripheral nervous system, whereas the Halverson response is related to the cortical or anterior growth of the central nervous system. This anterior growth is known as the process of telencephalization (**22**, p. 494). The anteroposterior development of the trunk and spinal axis,[3] the proximodistal growth of the peripheral nervous system, and telencephalization of the central nervous system constitute the main features of what the writer has called the radial hypothesis of development. This hypothesis indicates the directional mode of development of specific responses from early mass activity. These assumptions, for the writer, are part of the organismic view of differentiation.

Finally, McGraw (**20**, p. 301) has accused the writer of a superficial enthusiasm for this hypothesis. "Scientists sometimes wax a little too enthusiastic over a particular pet theory. Anxious to make the theory all inclusive they are occasionally guilty of overlooking salient facts. It appears that Irwin has fallen into such a pitfall in his enthusiasm over the organismic hypothesis."

It is true that occasionally scientists incautiously project their theoretical interpretations beyond the established facts, when they should look upon their hypothetical formulations merely as tools to advance their experimentation. Whether the present writer has enthusiastically fallen into this 'pitfall' as McGraw suggests can easily be verified or disproved by a casual reading of pages 388 and 392 of the article (**15**) under discussion. There a caution will be found concerning the necessity of experimental verification of the organismic analogy afforded to students of behavior by the biological sciences. Indeed, the express purpose of that article was to indicate types of experimentation which would determine

[3] An instance of the anteroposterior law is the fact, noted above, that the hind limbs develop both structurally and functionally in most animal forms later than the forelimbs.

whether or not the anatomical analogy does hold for behavior. The present reply, by no means exhaustive, attempts to further mobilize some of the outstanding studies on the problem. These evidently go a long way in establishing the analogy for a part of the hypothesis, namely, the proximodistal development of behavior. When verified evidence of the contrary position is forthcoming, the writer, of course, will be forced to relinquish the theory whether it is a pet one or not. Meanwhile, there is sufficient if not complete evidence for its use as an experimental tool in the field of infant behavior.

REFERENCES

1. Angulo y González, A. W., The prenatal development of behavior in the albino rat, *J. Comp. Neurol.*, 1932, **55**, 395–442.
2. Arey, L. B., Developmental anatomy: A text-book and laboratory manual of embryology, Philadelphia: W. B. Saunders, 1931. Pp. ix, 563.
3. Bardeen, C. R., & Lewis, W. H., Development of the limbs, body-wall and back in man, *Amer. J. Anat.*, 1901–1902, **1**, 1–35.
4. Chaney, L. B., & McGraw, M. B., Reflexes and other motor activities in newborn infants, *Bull. Neurol.*, Inst. of New York, 1932, **2**, 1–56.
5. Coghill, G. E., Correlated anatomical and physiological studies of the growth of the nervous system in amphibia: VI. The mechanism of integration in Amblystoma punctatum, *J. Comp. Neurol.*, 1926, **41**, 95–151.
6. Coghill, G. E., Anatomy and the problem of behaviour, New York, Macmillan, 1929. Pp. xii, 113.
7. Detwiler, S. R., Experiments on the transplantation of limbs in Amblystoma: The formation of nerve plexuses and the function of the limbs, *J. Exper. Zool.*, 1920, **31**, 117–169.
8. Detwiler, S. R., Experiments on the transplantation of limbs in Amblystoma: Further observations on peripheral nerve connections, *J. Exper. Zool.*, 1922, **35**, 115–161.
9. Halverson, H. M., An experimental study of prehension in infants by means of systematic cinema records, *Genet. Psychol. Monog.*, 1931, **10**, 107–286.
10. Harrison, R. G., Further experiments on the development of peripheral nerves, *Amer. J. Anat.*, 1905–1906, **5**, 121–131.
11. Harrison, R. G., Experiments in transplanting limbs and their bearing upon the problems of the development of nerves, *J. Exper. Zool.*, 1907, **4**, 239–281.
12. Harrison, R. G., Embryonic transplantation and development of the nervous system, *Anat. Rec.*, 1908, **2**, 385–410.
13. Irwin, O. C., The organismic hypothesis and differentiation of behavior. I. The cell theory and the neurone doctrine, Psychol. Rev., 1932, **39**, 128–146.
14. Irwin, O. C., The organismic hypothesis and differentiation of behavior. II. The reflex arc concept, Psychol. Rev., 1932, **39**, 189–202.
15. Irwin, O. C., The organismic hypothesis and differentiation of behavior. III. The differentiation of human behavior, Psychol. Rev., 1932, **39**, 387–393.
16. Irwin, O. C., Dennis on mass activity: A reply, Psychol. Rev., 1933, **40**, 215–219.

17. KEIBEL, F., & MALL, F. P. [editors], Manual of human embryology, Philadelphia: J. B. Lippincott, Vol. 1, 1910. Pp. xviii, 548; Vol. 2, 1912. Pp. viii, 1032.
18. KUO, Z. Y., Ontogeny of embryonic behavior in Aves. I. The chronology and general nature of the behavior of the chick embryo, *J. Exper. Zool.*, 1932, 61, 395–430.
19. LEWIS, W. H., The development of the arm in man, *Amer. J. Anat.*, 1901–1902, 1, 145–183.
20. MCGRAW, M. B., Grasping in infants and the proximo-distal course of growth, PSYCHOL. REV., 1933, 40, 301–302.
21. MINKOWSKI, M., Über früzeitige Bewegungen, Reflexe und muskeläre Reaktionen beim menschlichen Fötus und ihre Beziehungen zum fötalen Nerven und Muskelsystem, *Schweiz. Med. Woch.*, 1922, 3, 721–724; 751–755.
22. TILNEY, F., & RILEY, H. A., The form and functions of the central nervous system: An introduction to the study of nervous diseases, New York: Paul B. Hoeber, 1923. Pp. xxvi, 1019.
23. WEISS, P., The relations between central and peripheral coördination, *J. Comp. Neurol.*, 1926, 40, 241–251.
24. WINDLE, W. F., & GRIFFIN, A. M., Observations on embryonic and fetal movemovements of the cat, *J. Comp. Neurol.*, 1931, 52, 149–188.

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DISCUSSION

IN DEFENSE OF THE WHALE—EMOTION IS AT LEAST A TERM OF CONVENIENCE

Professor Meyer has recently [1] led us by an interesting bit of reasoning from the position that a whale is not a fish to a denial that the whale exists at all. Now, although we can readily agree with the first part of the argument, it would seem to be a much more difficult task to eliminate the whale from the picture entirely. It is to be remembered, in the first place, that all biological classifications are for convenience only and had some other criterion for classification been selected, perhaps the whale would have been a fish. And whatever the classification, the whale still takes up considerable space in the sea.

Meyer's argument against the use of the term 'emotion' is that there is nothing in emotion that is not present in many other types of behavior. "Who," says Meyer, "draws a line between more and less flooding of the skin with blood?" and . . . "How does the child's fear of the dark differ from my own fearless (?) attitude when I walk home on a lonely city street in the dark, and, in passing a stranger, quite naturally assume a slightly cautious attitude, or from my own highly tense attitude when in a far-away country on a deserted highway I once was held up, but succeeded in bluffing my two adversaries into thinking that I feared them less than they feared me—almost to collapse when ten minutes later I felt safe? In none of the three cases is there absence of function of the skeletal muscles, the vascular muscles, the glands, the nervous system. There are differences in the intensity of function, but since nothing is totally absent, where does the fear begin?"

But is it true that nothing is totally absent? Is the stranger present in the child's situation? Was the darkness present on the deserted highway? Certainly any attempt at the explanation of human behavior without a complete understanding of the external and internal stimulating situations dates back to a psychology as old as that of the will.

[1] M. F. Meyer, That whale among the fishes—the theory of emotions, PSYCHOL. REV., 1933, 40, 292–300.

Should Meyer's argument hold generally, it would become necessary to eliminate a great many classifications which certainly have aided in advancing our knowledge in both the natural and social sciences. Let us admit that there is no line to be drawn between the more and less flooding of the skin with blood. Similarly, who draws a line between plants and animals? Or between whooping cough and laryngitis? Or between cold and hot water? Or between the rat and man—both have eyes, ears, nose, hair, claws, nervous systems, glands, digestive systems and even mayhap, a tail. 'Since nothing is totally absent'—where does the man begin? (Perhaps indeed we have here the explanation for the predominance of the rat in psychology.) In the border line cases, it is quite impossible perhaps to make the distinction between classes, but for ordinary, to say nothing of extreme situations, distinctions may be very valuable. In the minds of some it will probably be expedient to use the terms 'rat' and 'man' for some time to come.

It is possible, too, that even though every element is present, nevertheless the arrangements of these elements and the intensity with which they function may result in conditions sufficiently different from each other that it will be a matter of at least scientific convenience to designate them by different terms. We learn in chemistry, for example, that the different arrangements of the same elements produce substances which exhibit widely different properties.

Whether or not emotions exhibit sufficient degrees of differentiation to justify classifying them by themselves is likely to be a matter of dispute for sometime. But it hardly seems logical that they should be eliminated merely because in emotional behavior some organs are active which are active in some other behavior also. The total behavior pattern of one who is extremely 'angry' may be almost unrecognizably distinct from that of the same man 'calmly' at work at his desk—and while all the elements in his blood stream may be there in each condition, there proportions are so changed as to be easily identifiable chemically. A bodily condition which after an encounter with a holdup man brings the organism of a strong man 'almost to collapse' is certainly profoundly different from the normal condition of that man while talking to a friend. Do not these widely diverging conditions justify study—and if they do, is it not

a matter of convenience to have a special term with which to designate them? The whale may not be a fish—but whaling is still a profitable occupation.

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